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SOME OF THE LESSONS OF NEUROTOMY.

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It is not very many years since all that we knew of nerve sections, and their results, could have been put into a very small space; but the increasing care with which the modern physician studies his cases has led in this, as in all directions, to novel and fast accumulating conclusions of the utmost practical value.

The sad opportunity of our late civil war afforded an unequalled chance to study this interesting subject, and a large experience has since added new opportunities. Out of this constant accumulation have come numerous essays, scattered here and there in journals, and embodying many facts of value which it is perhaps worth while now to bring together. Moreover, I have of late learned in regard to certain nerve sections a class of facts which, I believe, has hitherto altogether escaped notice. I shall deal, therefore, in this paper with the subject of neurotomy under two heads:—

1. The indications for nerve sections.
2. The consequences of nerve sections.

In discussing these subjects I shall of course avoid, as far as

possible, what is already old or commonplace medical knowledge, and shall dwell more largely on those points which are still novel, and many of which have neither found their way into the text books nor become familiar to the surgeon, as part of the light in which he should consider the propriety of a nerve section.

1. *Indications for Neurotomy.*

It becomes needful to discuss the question of a nerve section whenever a local irritation, either arising from disease[1] or from mechanical injury, gives rise to unmanageable peripheral symptoms (such as spasm, hyperalgesia, neuralgia), or to reflex neuroses of like nature affecting distant parts, or disturbing the circulation in the centres, as in the rare causation of vertigo.

Most of the questions which arise in this connection are easily answered; but there are certain points in regard to the various forms of pain which will bear some further examination, because their clinical history is such as to make it at times very difficult to decide for or against the operation of nerve section. Thus in the common case of neuralgia of the fifth nerve, we usually find the pain at first confined to a single branch of the nerve, and we have no evidence to make us sure that the cause of pain lies in the peripheral distribution of this branch. Indeed, we have as often reason to believe that it does not, and that the centre or the ganglion is at fault; but even when this is the case it is frequently clear that the peripheral expansion, owing to the state of the centre, has passed into a condition of hyperæsthesia, so that, whether or not the original cause lay in the surfaces, these have become secondarily the parents of pain, and in turn react on the sensitized centre. When, therefore, other means have been exhausted, I am frequently willing to cut a facial nerve branch, whether or not I am sure that the original cause of pain lies in the periphery. Later in the history of facial pain the matter is complicated by the spread of pain to other branches of the nerve; but even then, when I am clear as to

[1] Case of Section of Brachial Plexus, Dr. Maury, *Amer. J. Med. Sci.*, 1875.

the filaments in which it first appeared, I am willing to divide these filaments, and in my experience I find that I get, even under such circumstances, a fair percentage of success. The operation is here justified by experience—by the fact that the section in some way alters the central pain-making conditions; and lastly, because nerve sections in the face give rise to little inconvenience, and to none of the curious consequences which so often follow upon sections of the nerves of the hand. The question of the propagation of pain from branch to branch in a great nerve, is one to which I shall return at another place. I think we must concede, however, that this diffusion of pain is not always evidence of the increase or spread in the centres of an organic cause, but is due rather to what I might call, using a photographer's phrase, a *sensitizing* of the related ganglionic cells, so that in neuralgia, as in tetanus, the capacity to produce abnormal pain is capable of passing from cell to cell, and sometimes of thus affecting distant centres. Here at least, in a remarkable degree, pain is the parent of pain. In the extremities, and never, so far as I know, in the face, the propagation of pain is occasionally due to an ascending neuritis, resulting, in rare cases, in a form of sclerotic alteration in the nerves, the sheaths of which it reaches in its upward march.

I have reported elsewhere a remarkable example of this change, which I saw with Dr. Mears. I believe it less frequent than I once supposed it to be.

Among the most important matters to consider in regard to the propriety of a nerve section, is this very question of the possibility of organic change having extended up the nerve trunk. It is a grave question, because it is well for success in neurotomy to make the section above, or at all events near to, the limit of such change; and I have seen, over and over, failures on account of lack of care in this matter. In neuralgias without known cause the nerve is rarely visibly altered; but in the terrible traumatic neuralgias which fall on the hand, and usually on the median nerve, there is apt to be more or less extension of neuritis from the seat of injury. Save in very coarse changes, it is difficult to determine the state of the nerve at the time of

operation; and mere inspection is not enough, since even the most practised touch will fail to settle a question which only the microscope can decide.

If the change be slight, we can afford to wait, since cutting off the irritating region is then competent, in most cases, to put the nerve in a state of rest above the line of section; and when this result is attained, the organic changes gradually disappear. When, however, the sclerosis is notable, it rarely fades away, but seems to have acquired permanent power to propagate itself centrally.

I find that physicians are apt to believe that tenderness of a nerve-trunk above the point of irritation is a sign of organic change in the nerve; but this is by no means true. In the early stages of ascending neuritis the nerve is horribly tender, but in an advanced sclerosis the sensibility is not nearly so great. In the vast bulk of local nerve lesions of the hand there is, however, the most notable sensitiveness of the main trunk; in fact, there is always, after a time, in such cases, hyperalgesia, and this is of two degrees. In the lighter form the nerve-trunk is not remarkably tender, but pressure causes increased pain in the peripheral distribution in and beyond the point of traumatic disturbance. In the later and more severe form, pressure causes pain in the nerve itself, and also in the area of the neuralgia.

In these horrible traumatic neuralgias, electricity is of no value. Even when galvanism applied to the nerve-trunk has the curious ability to suspend, during passage of a current in either direction, the hyperalgesia of the painful areas, the current has no permanent therapeutic power. I mention the fact here, because we might naturally suppose that a current, which used on the nerve-trunk annihilates morbid sensation for the time, and enables us to touch the most sensitive regions without causing pain, would be certain to cure, if used assiduously. Yet such is not the case, and I have seen no good result in traumatic neuralgia from the most industrious use of electricity.

I am quite unable to say why galvanism possesses anæsthetic power in one of these cases, and not in another. I saw a few years ago two severe injuries of the median nerves in the same

part of the palm. In both, the fingers were painfully sensitive to the slightest touch. I galvanized three inches of the median above the wrist, using thirty cells. While the current passed, the analgesia of the fingers ceased to exist. I then reversed the current, which had been peripheral in direction, and was amazed to find the same result. Even twelve cells gave me the same effect, but an induction current was valueless. In the second case galvanism produced no good effect.

An excitable state of the main nerve-trunks in nerve wounds must be due to a propagation of sensitizing influences upward, or else to an acquired condition of the related centres.

The most startling sensitizing of a nerve, both motor and sensory, is obtained by chilling with a freezing mixture a limited portion until touch is lost, when the portion below becomes so excitable that the least tap over the main trunk causes motion of all the muscles which the nerve supplies, and also some pain, so that while tactile impressions do not pass the frozen part, those of pain do; showing, I think, as do many other facts, the distinctness of difference between the forms of nerve excitement which are caused by touch and by pain.

Since every nerve contains a certain number of recurrent fibres which find their way to the brain along trunks other than the trunk into which they primarily run,[1] it might have been expected that the nerves to which they finally convey impressions would share in the sensitiveness I have described. Thus the radial probably receives recurrent fibres from median nerve areas; but as yet I have been unable to find that the radial becomes sensitive when the median itself has been hurt. It certainly does not acquire such sensitiveness early, and I have doubts as to whether it ever does. Perhaps, since the recurrent nerve supply multiplies as we approach the periphery, the absence of the phenomenon in question may depend on the position of injuries which usually affect the palm. Lesions of the finger ends in the median areas would be more apt to give us sensitiveness in the radial trunk.

[1] 'Injuries of Nerves,' p. 60, S. Weir Mitchell.

2. *Nerve sections, and their consequences.*

The first question which arises when we have decided to resort to neurotomy, is one which at first glance it seems absurd to raise. It is, what nerve we shall cut. In the face, the answer is ready enough; but in the complicated distributions of nerves in the hand, it is not always so easy. The anatomists have somewhat misled us by their statements as to the innervation of fixed areas by certain nerves: whereas it is plain from the numerous cases of Letievant and myself, that it is to the clinical inquirer anatomy must look for a knowledge of surface-areas of nerve distribution, and that these areas differ widely in individuals. The uncertainty arises out of the curious variation of nerve supply to the hand. In some animals all the nerves but one may be cut without loss of sensation anywhere; and while in man this is by no means so completely or constantly true, case after case has been reported in which section of the median scarcely disturbed sensation.

How much of this preservation of sensation is due to anomalies, and how much to extreme differences in the amount of recurrent nerve supply, cannot as yet be known.

When a local injury falls on parts which are innervated by two nerves, or when it alights on the doubtful boundaries of two nerve-territories, there may be a good deal of reason to hesitate as to a nerve section.

The best means in these rare cases is to learn which of the main trunks is sensitive to pressure and to electricity, and how far benumbing pressure influences the morbid phenomena. But surgeons are rarely careful enough to make a thorough study of their cases, and I have several times known them to cut the wrong nerve. In one case, an injury of the palm caused epilepsy. The wound traversed the median and radial region. I found that pressure on the wound of entry (median) caused no result, while pressure on the wound of exit (radial) gave rise to violent fits. Section of the radial apparently cured the fits.

In another instance, a wound of the palm occasioned intense tenderness of the fore-finger and third finger, and also pain on

the back of the fore-finger. This latter induced the surgeon to try if pressure on the median and musculo-spiral would influence the pain. It gave rise to greatly increased suffering when applied to the former; but benumbing pressure by a tourniquet over the musculo-spiral not only annihilated the pain on the back of the fore-finger, but also in the median palm territory. He cut the musculo-spiral: the whole of the pain departed, only to come back within a week in a scarcely mitigated shape. The pain was relieved, as is pain in the infra-orbital nerve by removal of a sound tooth not in its territory. I cured the neuralgia by cutting the median. If, in this case, the surgeon had kept up the pressure on the median, by etherizing the patient so as to enable her to bear the force needed, he would not have made so grave a mistake. The tenderness of the median was in itself the true guide.

The operation of neurotomy is a simple one; but, as its object is to secure the permanent cutting off of a region of disease, the main point is to make sure that the exsected portion of nerve shall not renew itself. I do not yet know how this is to be done; and, after a long and large experience, I find that the capacity of nerves to remake their lost parts is almost invincible, and is the true cause of many of our failures to attain permanently the object in view—the relief of pain.

If the pain recurs within five months, it is likely that the cause lies in more than one nerve, or that the trouble is, or has become, a central one. Nearly always a section gives relief for a time. If, at the close of five months, the areas cut off begin to be sensitive, and to regain *surface sensibility to touch*, the lost nerve-tissue is being remade; and if, after a time, electricity—interrupted or reversed, galvanic or faradaic—applied above the line of section causes motion in the muscles cut off by the neurotomy, it is of course put beyond doubt that the nerve has been yet more thoroughly restored. The return of function is always in this order: sensation first, motion last. I have tried many devices to prevent reunion, but none have answered. Doubling the lower end of the cut nerve, turning the ends aside, imbedding one or both in other tissues—one and all fail. The length of nerve removed has been supposed to be the most efficient factor in retarding or

preventing reunion; but we need a careful study of this matter in animals to tell us to what extent separation of the nerve ends puts off the date of return of function, since there is some reason to suspect that mere section is as efficient as exsections which remove less than two inches. I mention it as a partly open question.

In the cases of secondary section, where the lower end of the cut median had been doubled on itself, reunion took place in eighteen months, and I could not distinguish the doubling in the large button formed, and out of which ran the delicate connecting filaments. One plan yet remains for trial, and that is, to divide the nerve and draw the ends past one another. This plan I am about to test in the dog, but I do not feel any confidence that it will succeed in preventing reunion.

To my mind, nothing is more strange than the bewildering certainty with which these nerve ends, however dealt with, succeed in threading the tissues between and reuniting.

The rate and sureness with which this process occurs is inversely as the age. Dr. Hodge's case of reunion of the radial is, I think, the briefest in time. Two inches of the nerve were removed.[1] In six weeks pain returned—a sign of little value for reunion if it stand alone; but in nine or ten weeks tactile sensibility came back. Within eleven months the nerve was cut a second time, and found to have been thoroughly remade.

The indication then is, in every case to take away as much of the nerve as possible, avoiding as far as may be sections which involve motor nerves.

The consequences of neural resections vary somewhat with the nerve on which we operate, since, as I have already said, it is exceptionally rare to see nutritive disturbances in facial neurotomies, and common to observe them in the hand.

The first result of a nerve section is the almost sure relief of the neural pain. This consequence may be temporary or permanent; and it may be delayed for a brief period, and then be gradual and more or less complete, these variations depending on causes already discussed. 2. The temperature rises in the area of distribution of the nerve, and in the course of months falls below the normal standard. I first showed

[1] Neurotomy, S. Weir Mitchell, *Amer. J. Med. Sci.*, April 1876.

this in regard to the nerves of the hand, and have over and over seen it illustrated in later cases. Whether it be true or not of the facial nerves I do not know, but shall learn on the earliest occasion. The proportion of vaso-motor nerves which reach a given area of face or hand by the nerve tracks of sensation, by the vessels and otherwise, may vary so as to efficiently affect the thermal phenomena, which follow nerve sections.

Nutritive alterations in the skin, nails, hairs, areolar tissues, and muscles, follow in a certain proportion of nerve sections. I have never seen any of these disorders after section of any branch of the fifth nerve. They are more common after resection of nerves which supply the feet. In the hand they are rarely seen when the radial has been cut, not often after ulnar section, but more often after divisions of the median nerve.

The favourite theory, that the parts rendered insensible are thereby exposed to mechanical lesions, does not answer to explain these facts, since they occur unequally in different nerves, and are seen in parts which have been zealously protected from harm.

Moreover, they seem to contradict the law of Brown-Séquard which has been so generally accepted, to wit, that partial or irritative lesions give rise to trophic changes, and that paralytic lesions occasion only such results as are due to prolonged inaction. This principle is commented upon with approval by Charcot in his recent lectures (*Syd. Trans.* p. 19), where he puts it in the following very distinct form: "At the outset I shall establish what I consider to be a fundamental distinction between these traumatic lesions of nerves: 1. The lesion sometimes consists of a clean and complete section, in which case the effects are merely, speaking generally, those resulting from the absence of nerve action; 2. Sometimes, owing to contused or lacerated wounds, the lesion is of such a nature as to set up irritation in the nerve, in which case, and then only, we see arise those trophical troubles to which I invite your attention. The disorders, following irritative (partial nerve lesions) are, affections of the skin, bones, joints, nails, hair, and *rapid* muscular atrophy."

"It may also be added (p. 37, note), that irritative lesions of muscles have been of late recorded by competent observers as following section or excision of mixed or purely motor-nerves."

These troubles were, however, limited to muscles and their connective tissues; and so far, with the above exception, no one seems to have dwelt on the possibility of the same kind of lesions following complete sections as follow those which are partial, and therefore presumably irritative.

Until recently I have myself admitted the correctness of Brown-Séquard's law, and for many nerves, as, for example, those of the face, we may still adhere to it; but as regards the hand nerves, and especially the median nerve, I shall now have occasion to prove that irritative trophic lesions are sometimes seen after entire section, and that, in a certain proportion of cases, complete section after irritation appears to give rise to, or permit the continuance of, somewhat anomalous forms of pain in the peripheral distribution of the cut nerve.

Section of median nerve at wrist by glass wound: healthy man, æt. 47; no history of syphilis; entire absence of sensation in median territory. From the eighth week frequent bullæ, followed by ulcers of the palmar face of the fore and middle finger; thickening and considerable enlargement of middle finger. The appearance of the ulcers, which when I saw him were numerous, was very singular. They resembled deep holes, cut out by a punch, and were almost dry, having, in fact, only the slightest discharge.

B. H., æt. 17: glass-cut; section, in palm, of median branches of fore-finger; complete insensibility of this part; the last inch of the fore-finger is purplish, excessively wasted, shining, and smooth. At the seventh day, the hand having been carefully guarded, a bulla broke out on the fore-finger, and left the whole of the face of the member ulcerated. The nail fell off, and after the ulcer healed frequent bullæ occurred. The new nail is deformed.

In these cases the sections were complete. In two others, in which there were incomplete lesions of the palmar median nerves, the trophic disorders were joint lesions, herpetic

eruptions, minute ulcers, shining skin, and deformed nails, and loss of hair on last phalanx dorsal aspect (one case). In both, median nerve exsections, to relieve pain, were followed by continuance of the trophic changes, and by frequent bullæ in one case. Entire section is not therefore always competent to put an end to irritative lesions caused by imperfect sections.

In one other case of complete median section, I have seen necrosis of the last phalanx of the fore-finger without exposure to violence.

It must be admitted that in the vast majority of cases of complete sections no marked trophic changes occur, but that in a small per centage these do certainly follow. The interest of the matter lies in the fact that they never are seen in the face.

I may be permitted again to say that most of the cases quoted were under my immediate care, and that the trophic accidents were not due to traumatic causes. Moreover, in some of them the bullæ and ulcers continued to appear for months.

The second group of facts to which I desire to call attention is still more remarkable. I have so far observed it only in median sections, and altogether but twice. Since I first observed the phenomenon in question I have had but one occasion to cut the median. It may therefore be exceptional or common.

The facts of the first case were these:—

Traumatic injury of median nerves in palm, in girl of 17 years. Intense pain in peripheral median distribution on motion of hand; total insensibility to pain and touch in fore-finger median regions, partial in rest of median areas.

Section of median at third month, 3 inches above wrist—removal of one inch of nerve. At the eighth day after operation I examined the hand with care, and found the following conditions. Again and again since, the same study has been made; and soon after my first inspection, Dr. Morris Lewis made the careful diagrams, which explain far better than any description in words can do the character of the sensory changes.

Between the dotted line — · — · — · (Diagrams 1 and 2) and the continuous line ———, lies a space within which the

sense of touch to a light impact (feather) was lessened or indistinct. The region far more than covers the usual area of median distribution; and in fact, as I have already said, nothing can be more uncertain than this territory, which nevertheless the Anatomies describe as rigid. Clinical sections enable us to see that it may extend to the middle of the third finger (medius) in one case, to the whole medius in another; to the half also of the fourth finger (ring) in a third instance; while in yet another case, the median was found to innervate in part even the ulnar side of the fourth digit.

Whether this extensive defect be due, in the present case, to an unusual distribution of median fibres, or to a reflex disturbance of central perceptivity, owing to the influence of the section, I cannot say.

Within the continuous line the sense of pain should, according to our old ideas, be annihilated, as it incloses territory most of which is usually supposed to be fed by the median nerve. But let us see what a curious state of things does really exist.

The areas, which are darkened (stippled), and which on the dorsum (Figs. 1 and 2) are far more extensive than those commonly innervated by the median nerve, have neither sense of touch, pain, nor temperature. Heat and cold, needle-pricks, pinching, and deep pressure, are alike unfelt. The singular distribution of this analgesic area will be noticed. It is seemingly most eccentric, and does not correspond to any special distribution of separate branches of the median nerve.

There are also within the continuous line two areas, the one marked by lines running in the axis of the limb, the second by transverse lines. The former territory has no tact-sense; but if the skin be pinched with a small forceps, or indented by the pressure of a blunt point, or firmly by the finger, the result is most agonizing pain, so that the patient may cry out.

In the area transversely marked, the pinching is not felt; but firm deep pressure causes pain; and in both territories the site of the pain is fairly well localised. In both of these areas the altered skin-spaces are insensible to the prick of a very fine needle, if this be oiled and introduced laterally into

Fig. 1.

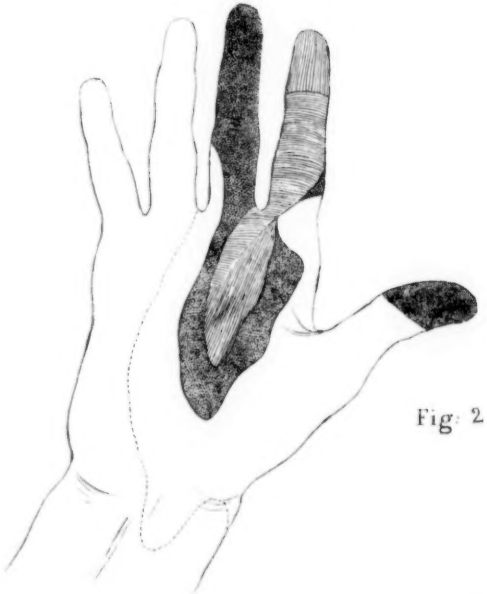

Fig. 2.

the skin so as not to indent it roughly. If roughly or deeply buried in the skin, it causes pain. It should be added, that the patient was securely blindfolded during the process of testing.

The second case is yet more difficult to explain. Its history is, in brief, this:—

Miss T., at the age of 2½ years, ran a splinter into the right palm, where the median branches. The wound healed over it. At the age of 22 years, Miss T. bruised the region in question; severe pain ensued in the thumb, fore and middle fingers. Dr. Sapolini, of Milan, removed the splinter, Oct. 1871. It was lying among the nerve filaments, and its removal was followed by exquisite torment, with causalgia and hyperalgesia.

In Sept. 1871, Dr. Sapolini, finding that pressure on the median trunk made the pain worse, and that pressure on the musculo-spiral nerve eased it, cut out one inch of the latter nerve—result, no pain for eight days; then return of all the symptoms. The extensor masses of the fore arm wasted rapidly, and wrist drop of course ensued.

Feb. 1874: section of median low down in the fore arm. Result, in fourteen days entire relief, and anæthesia and analgesia of median regions of thumb, fore finger, and up to an irregular line on palmar face of median finger. Tact was disturbed over a wider area, including the lower part of the whole palmar face of the ring finger.

In March 1876 there was great pain in and below the line of section of the musculo-spiral, and in its dorsal distribution on the hand. The extensors were as large as usual, and acting well. After much fruitless medication, I had the musculo-spiral examined at the point cut. It was thoroughly remade. Two inches were removed, including two large terminal buttons, between which ran the new nerve. Results, paralysis of extensors, and relief to this date, June 1, 1878.

In January 1877 Miss T. came to me complaining of a return of all the old pains in the median areas. Sensation was partly restored, and I could move the median thumb muscles by galvanising the main trunk above the line of section, although whether she could also effect this by will was

doubtful. It was but too plain that the median had partially remade its mischievous connections.

In May 1877 Dr. Brinton cut down on the median, and found large buttons as usual, but the filaments between were scarcely perceptible. Accordingly, I lifted the central portion above the upper button, wiped it dry, and put it on a piece of caoutchouc; then I faradised it gently, when the median thumb muscles all moved freely. The nerve was then cut, and three inches removed.

After two weeks she was well enough to be examined with care, when I obtained the following singular facts, as shown in Diagrams Nos. 3 and 4.

Within the dotted line, and up to the continuous line, touch was impaired in various degrees, most on the middle finger. The area differs remarkably from that of the loss in Case 1.

In the areas of the diagram which are striated in the line of the limb there was very marked impairment of touch, and the most exquisite pain on pinching and on pressure, while the needle was painful only when carried in deeply below the skin.

The areas marked with transverse lines had no sense of tact, and did not feel pain when lightly—not deeply or largely—pinched, but were subject to intense pain on deep pressure with a blunt point.

The two cases here given are valuable, chiefly because they have been studied with unusual care. Most of such opportunities fall into the hands of surgeons, and the histories are anything but complete. Even of the later cases this is true; and I can scarcely recall a case in which the amount, character, and area of sensation-loss are stated fully and clearly. In my own examples we must remember that the median nerves in the palm had been subjected to prolonged irritation; then came section, and complete relief of active pain.

I shall first consider Case 1. We find what still exists unaltered, extensive loss of touch, with loss of the previously existent hyperalgesia of the surface; while in certain areas, different in each case, and most eccentric in their boundaries, we have two curious states:—

1st. Intense over-sensitiveness of subcuticular tissues, as shown by pain on pinching and pressure; and

Fig. 3.

Fig. 4.

2nd. More or less power to state the locality of the pain.

It is probable that my cases are not exceptions, and how shall we explain these facts? It is quite possible that like states existed before the operation; but this is hard to know, on account of the pain then given by an examination; but why should the operation, which annihilates touch, leave pain-power unaltered in the regions under the skin?

It is quite sure that there are frequent anomalies in nerve distribution, so that as a consequence the median gets more ulnar fibres than usual in the lower half of the palm; but this also is incapable of explaining the facts, since some of the new pain-areas are in the thumb, which has no such fibres in any case, while we find like pain areas also on the dorsal aspect of the fingers.

Moreover, why should such untouched connections not leave us areas of touch, even if lessened touch? and why should they only preserve or permit the parts to acquire an abnormal sub-surface sensitiveness to pain?

We have, of course, to believe that there remain connections of some kind with the centres: and we are obliged, I think, to fall back for explanation upon the recurrent nerve fibres, which Bernard, Arloing, and Trippier have shown to exist so widely.

The function of these nerves is as yet unknown, but the present cases show, I believe, that whatever be the normal activities of recurrent fibres, they may become the means both of feeling pain and of enabling us to localise it.

I am inclined to think, although I am not sure, that the changes which sensitize these fibres arise, at least in part, out of the previously existing irritations; but this is a question which can only be decided by future cases. If due to the section, the new sensitiveness may be a consequence of altered vascular states caused by the operation, since it is unlikely that the Wallerian degeneration of the neighbouring fibres could affect the recurrent fibres, which are left practically untouched, and have been sometimes seen to be unaltered in parts of nerves below the section. This alone seems sure, that the primary irritation or the section so influence the recurrent fibres, which reach the centre through the ulnar and radial connections of the median, as to give them new and

abnormal capacities to feel pain. At all events, these cases seem to open a novel chapter in neural pathology.

The second case is more complicated, because the musculo-spiral had been cut a year before the median. Yet not only was there the same phenomenon of pain areas, but a remarkable preservation of touch, far greater, in fact, than after the first median section. It is but too probable that already repair had begun in the musculo-spiral, since otherwise the preservation of sensitiveness to touch and pain rests inexplicable, even with the most liberal view of possible ulnar anomalies.

And now to return for a moment to Brown-Séquard's law. I have said that the present facts seem to contradict it. Certainly this is true of the simple nerve sections first related; but is it also true of the last a 1 more remarkable cases?

We have here irritation long continued, intense peripheral pain and analgesia. We cut the main nerve, and pain areas still exist, inexplicable as due to coarse anastomoses. If we pre-suppose a descending neuritis, it would involve recurrent nerves as they enter the branches below the irritated point. Section of the nerve would of course fail to influence these recurrent fibres; but by raising the temperature, and though the local vaso-motor section induces palsy, it might well add fresh causes of irritation to those already affecting the recurrent fibres.

The latter of the cases I have adduced would not, therefore, contradict the law of Brown-Séquard.

In both of the cases last quoted, the areas in which pain was caused by deep pressure are insensitive to heat and cold as such, but both give rise, after a few moments' contact, to sense of dull pain. We may infer, therefore, that in these instances whatever fibres carried impression of pain and, in one case, of touch, were unable to perform the same office for thermal impressions.

In this condition I leave for the present this most puzzling problem in neuro-pathology. I trust that the chances offered to solve it through accidental sections, where there has been no previous irritation, will not be neglected.

The practical question which these phenomena leave to us is, whether it is not best at all risks to open the palm, and

remove the portions of irritated nerve-fibres, when section above them has failed to relieve pain. The two hands I have described are now useless, owing to the sensitiveness I have spoken of. There is no spontaneous pain, but the member cannot be used, since all pressure gives rise to intense distress.

Among the consequences of sections which I do not find mentioned, are the remarkable hyperalgesia in and near the line of wound; and, what I have seen thrice, troublesome and repeated herpes along the lips of the yet unclosed wound. Irritable and obstinate ulceration also attends the healing of these wounds in a small proportion of the cases.

Repeated section of the same nerve has been several times needful in my experience. It has been called for in these cases by return of the pains which were relieved by the first section.

If, however, I have correctly studied and reported the two cases last stated, we may come face to face with a hitherto unrecognised difficulty. Pain exists: we cut the main nerve; relief of spontaneous pain ensues, but after a brief time we have pain on pressure. If now at a remoter date this increases, and spontaneous pain be added, how shall we feel sure that the increase is due to a remade nerve? The test of increased surface-sensibility to touch is the earliest reliable functional evidence of such reconstruction, and precedes the return of muscular manifestations. It is, however, a less certain test than the diminution of atrophy in the muscular masses, and the capacity to move the muscles by currents, which include portions of the cut nerve above the line of section. This latter test is absolute, and enables us to feel sure that the nerve has been remade, and that a second operation has become justifiable. Whether it precedes or not the revival of will-power over the muscles, I have not been able as yet to determine with certainty; but I am inclined to think that it does anticipate in some cases the normal functional manifestations.

That the skin-sense returns before the muscles regain volitional or electrical activity depends, I presume, upon the fact that the nerve must be remade before the muscle can be reconstructed, and that until it is so reconstructed it cannot begin to respond to volitional or electrical stimulus.

ON AFFECTIONS OF SPEECH FROM DISEASE OF THE BRAIN.

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IT is very difficult for many reasons to write on Affections of Speech. So much, since the memorable researches of Dax and Broca, has been done in the investigation of these cases of disease of the brain, that there is an *embarras de richesse* in material. To refer only to what has been done in this country, we have the names of Gairdner, Moxon, Broadbent, William Ogle, Bastian, John W. Ogle, Thomas Watson, Alexander Robertson, Ireland, Wilks, Bristowe, Ferrier, Bateman, and others. To Wilks, Gairdner, Moxon, Broadbent, and Ferrier, I feel under great obligations. Besides recognising the value of Broadbent's work on this subject, I have to acknowledge a particular indebtedness to him. Broadbent's hypothesis—a verified hypothesis—is, I think, essential to the methodical investigation of affections of speech. Let me give at once an illustration of its value. It disposes of the difficulty there otherwise would be in holding (1) that loss of speech is, on the physical side, loss of nervous arrangements for highly special and complex articulatory *movements*, and (2) that in cases of loss of speech the articulatory *muscles* are not paralysed, or but slightly paralysed. I shall assume that the reader is well acquainted with Broadbent's researches on the representation of certain movements of the two sides of the body in each side of the brain; the reader must not assume that Broadbent endorses the applications I make of his hypothesis. The recent encyclopædic

article on Affections of Speech, by Kussmaul, in Ziemssen's 'Practice of Medicine,' is very complete and highly original. It is worthy of most careful study.

The subject has so many sides—psychological, anatomical, physiological, and pathological—that it is very difficult to fix on an order of exposition. It will not do to consider affections of speech on but one of these sides. To show how they mutually bear, we must see each distinctly. For example; we must not confound the physiology of a case with its pathology, by using for either the vague term "disease." Again, we must not ignore anatomy when speaking of the physical basis of words, being content with morphology, as in saying that words "reside" in this or that part of the brain. Supposing we could be certain that this or that grouping of cells and nerve-fibres was concerned in speech, from its being always destroyed when speech is lost, we should still have to find out the anatomy of the centre. Even supposing we were sure that the psychical states called words, and the nervous states in the "centre for words," were the same things, we should still have the anatomy of that centre to consider. The morphology of a centre deals with its shape, with its "geographical" position, with the sizes and shapes of its constituent elements. A knowledge of the anatomy of a centre is a knowledge of the parts of the body represented in it, and of the ways in which these parts are therein represented. Whilst so much has been learned as to the morphology of the cerebrum—cerebral topography—it is chiefly to the recent researches of Hitzig and Ferrier that we are indebted for our knowledge of the anatomy of many of the convolutions, that is, a knowledge of the parts of the body these convolutions represent. It is supposed that the anatomy of the parts of the brain concerned with words is that they are cerebral nervous arrangements representing the articulatory muscles in very special and complex movements. Similarly, a knowledge of the anatomy of the centres concerned during visual ideation is a knowledge of those regions of the brain where certain parts of the organism (retina and ocular muscles) are represented in particular and complex combinations. A merely materialistic or morphological explanation of speech or mind,

supposing one could be given, is not an anatomical explanation. Morphologically, the substratum of a word or of a syllable is made up of nerve-cells and fibres: anatomically speaking, we say it is made up of nerve-cells and fibres representing some particular articulatory movement.

Unless we most carefully distinguish betwixt psychology and the anatomy and physiology of the nervous system in this inquiry, we shall not see the fundamental similarity there is betwixt the defect often described in psychological phraseology as "loss of memory for words," and the defect called ataxy of articulation. A method which is founded on classifications which are partly anatomical and physiological, and partly psychological, confuses the real issues. These mixed classifications lead to the use of such expressions as that an *idea* of a word produces an articulatory *movement;* whereas a psychical state, an "idea of a word" (or simply "a word") cannot produce an articulatory movement, a physical state. On any view whatever as to the relation of mental states and nervous states such expressions are not warrantable in a *medical* inquiry. We could only say that discharge of the cells and fibres of the anatomical substratum of a word produces the articulatory movement. In all our studies of diseases of the nervous system we must be on our guard against the fallacy that what are physical states in lower centres fine away *into* psychical states in higher centres; that, for example, vibrations of sensory nerves *become* sensations, or that somehow or another an idea produces a movement.

Keeping them distinct, we must consider now one and now another of the several sides of our subject: sometimes, for example, we consider the psychical side—speech—and at other times the anatomical basis of speech. We cannot go right on with the psychology, nor with the anatomy, nor with the pathology of our subject. We must consider now one and now the other, endeavouring to trace a correspondence betwixt them.

I do not believe it to be possible for any one to write methodically on these cases of disease of the nervous system without considering them in relation to other kinds of nervous disease; nor to be desirable in a medical writer if it were pos-

sible. Broadbent's hypothesis is exemplified in cases of epilepsy and hemiplegia, as well as in cases of affections of speech, and can only be vividly realised when these several diseases have been carefully studied. Speech and Perception ("words" and "images") co-operate so intimately in Mentation (to use Metcalfe-Johnson's term) that the latter process must be considered. We must speak briefly of Imperception—loss of images—as well as of loss of Speech—loss of symbols. The same general principle is, I think, displayed in each. Both in delirium (partial imperception) and in affections of speech the patient is reduced to a more automatic condition; respectively reduced to the more organised relations of images and words. Again, we have temporary loss or defect of speech after certain epileptiform seizures: temporary affections of speech after these seizures are of great value in elucidating some difficult parts of our subject, and cannot be understood without a good knowledge of various other kinds of epileptic and epileptiform paroxysms, and post-paroxysmal states. After a convulsion beginning in the (right) side of the face or tongue, or in both these parts, there often remains temporary speechlessness, although the articulatory muscles move well. Surely we ought to consider cases of discharge of the centres for words as well as cases in which these centres are destroyed, just as we consider not only hemiplegia but hemispasm. Before trying to analyse that very difficult symptom called ataxy of articulation, we should try to understand the more easily studied disorder of co-ordination, locomotor ataxy; and before that, the least difficult disorder of co-ordination of movements resulting from ocular paralysis. Unless we do, we shall not successfully combat the notion that there are centres for co-ordination of words which are something over and above centres for special and complex movements of the articulatory muscles, and that a patient can, from lesion of such a centre, have a loss of co-ordination, without veritable loss of some of the movements represented in it.

It might seem that we could consider cases of aphasia, as a set of symptoms at least, without regard to the pathology of different cases of nervous disease. We really could not.

It so happens that different morbid processes have what, for brevity, we may metaphorically call different seats of election; thus, that defect of speech with which there are frequent mistakes in words is nearly always produced by local cerebral softening; that defect which is called ataxy of articulation, is, I think, most often produced by hæmorrhage. Hence we must consider hemiplegia in relation to affections of speech; for it so happens that the first kind of defect mostly occurs, as Hammond has pointed out, without hemiplegia, or without persistent hemiplegia, a state of things producible by embolism and thrombosis, and the latter mostly with hemiplegia and persistent hemiplegia, a state of things usually produced by hæmorrhage. From ignoring such considerations, the two kinds of defects are by some considered to be absolutely different, whereas on the anatomico-physiological side they are but very different degrees of one kind of defect.

There are certain most general principles which apply, not only to affections of speech, but also to the commonest variety of paralysis, to the simplest of convulsive seizures, and to cases of insanity.

The facts that the speechless patient is frequently reduced to the use only of the most general propositions "yes" or "no," or both; that he may be unable to say "no" when told, although he says it readily in reply to questions requiring dissent; that he may be able ordinarily to put out his tongue well, as for example to catch a stray crumb, and yet unable to put it out when he tries, after being asked to do so; that he loses intellectual language and not emotional language; that although he does not speak, he understands what we say to him; and many other facts of the same order, illustrate exactly the same principle as do such facts from other cases of disease of the nervous system as—that in hemiplegia the arm suffers more than the leg; that most convulsions beginning unilaterally begin in the index finger and thumb; that in cases of post-epileptic insanity there are degrees of temporary reduction from the least towards the most "organised actions," degrees proportional to the severity of the discharge in the paroxysm, or rather to the amount of exhaustion of the highest centres produced by the discharge causing the paroxysm. In all

these cases—except in the instance of convulsion, which, however, illustrates the principle in another way—there are, negatively, degrees of loss of the most voluntary processes with, positively, conservation of the next most voluntary or next more automatic; otherwise put, there are degrees of loss of the latest acquirements with conservation of the earlier, especially of the inherited, acquirements; speaking of the physical side, there are degrees of loss of function of the least organised nervous arrangements with conservation of function of the more organised. There is in each reduction to a more automatic condition: in each there is Dissolution, using this term as Spencer does, as the opposite of Evolution.[1]

In *defects* of speech we may find that the patient utters instead of the word intended a word of the same class in meaning, as "worm-powder" for "cough-medicine"; or, in sound, as "parasol" for "castor oil." The presumption is that the patient uses what is to him a more "organised" or "earlier" word, and if so, Dissolution is again seen. But often there is no obvious relation of any sort betwixt the word said and the one appropriate, and thus the mistake does not appear to come under Dissolution. If, however, we apply the broad principles which we can, I think, establish from other cases of Dissolution, viz. from degrees of insanity—especially the slight degrees of the post-epileptic insanity just spoken of—we shall be able to show that many of the apparently random mistakes in words are not real exceptions to the principle of Dissolution.

For the above reasons I shall make frequent references to other classes of nervous disease. The subject is already complex without these excursions, but we must face the complexity. Dr. Curnow has well said (*Medical Times and Gazette*, Nov. 29, p. 616), "The tendency to appear exact by disregarding the complexity of the factors is the old failing in our medical history."

[1] Here I must acknowledge my great indebtedness to Spencer. The facts stated in the text seem to me to be illustrations from actual cases of disease, of conclusions he has arrived at deductively in his *Psychology*. It is not affirmed that we have the exact opposite of Evolution from the apparently brutal doings of disease; the proper opposite is seen in healthy senescense, as Spencer has shown. But from disease there is, in general, the corresponding opposite of Evolution.

Certain provisional divisions of our subject must be made. The reader is asked to bear in mind that these are admittedly arbitrary; they are not put forward as scientific distinctions. Divisions [1] and Arrangements are easy, Distinctions and Classifications are difficult. But in the study of a very complex matter, we must first divide, and then distinguish. This is not contradictory to what was said before on the necessity of encountering the full complexity of our subject. Harm comes, not from dividing and arranging, but from stopping in this stage, from taking provisional divisions to be real distinctions, and putting forward elaborate arrangements, with divisions and subdivisions, as being classifications. In other words, we shall, to start with, consider our subject empirically, and afterwards scientifically; we first arbitrarily divide and arrange for convenience of obtaining the main facts which particular cases supply, and then try to classify the facts, in order to show their true relations one to another, and consider them on the psychical side as defects of mind, and on the physical side as defects of the nervous system. Empirically we consider the cases of affection of speech we meet with, as they *approach* certain nosological types (most frequently occurring cases), scientifically we classify the facts thus obtained, to show how affections of speech are *departures from* what we know of healthy states of mind and body. The latter study is of the cases as they show different degrees of nervous Dissolution.

Let us first of all make a very rough popular division. When a person "Talks" there are three things going on—Speech, Articulation, and Voice. Disease can separate them. Thus from disease of the larynx, or from paralysis of its

[1] "How often would controversies be sweetened were people to remember that 'Distinctions and Divisions are very different things,' and that 'one of them is the most necessary and conducive to true knowledge that can be: the other, *when made too much of*, serves only to puzzle and confuse the understanding.' Locke's words are the germ of that wise aphorism of Coleridge: 'It is a dull or obtuse mind that must divide in order to distinguish; but it is a still worse that distinguishes in order to divide.' And if we cast our eyes back over time, it is the same spirit as that which led Anaxagoras to say, 'Things in this one connected world are not cut off from one another as if with a hatchet.'"—*Westminster Review* (art. Locke), January 1877 (no italics in original).

nerves, we have loss of voice, but articulation and speech remain good. Again, in complete paralysis of the tongue, lips, and palate, articulation is lost, but speech is not even impaired; the patient remains able to express himself in writing, which shows that he retains speech—internal speech —that he propositionises well. Lastly, in extensive disease in a certain region in one half of the brain (left half usually) there is loss of speech, internal and external, but the articulatory muscles move well.

Let us make a wider division. Using the term Language, we make two divisions of it, Intellectual and Emotional. The patient, whom we call speechless (he is also defective in pantomime), has lost intellectual language and has not lost emotional language.

The kind of case we shall consider first is that of a man who has lost speech, and whose pantomime is impaired, but whose articulatory muscles move well, whose vocal organs are sound, and whose emotional manifestations are unaffected. This is the kind of case to be spoken of as No. 2 (p. 314).

The term Aphasia has been given to affections of speech by Trousseau; it is used for defects as well as for loss of speech. I think the expression Affections of Speech (including defects and loss) is preferable. Neither term is very good, for there is, at least in many cases, more than loss of *speech*; pantomime is impaired; there is often a loss or defect in symbolising relations of things in any way. Dr. Hamilton proposes the term Asemasia, which seems a good one. He derives it "from ἀ and σημαίνω, an inability to indicate by signs or language." It is too late, I fear, to displace the word aphasia. Aphasia will be sometimes used as synonymous with Affections of Speech in this article.

We must at once say briefly what we mean by speech, in addition to what has been said by implication when excluding articulation, as this is popularly understood, and voice. To speak is not simply to utter words, it is to propositionise. A proposition is such a relation of words that it makes one new meaning; not by a mere addition of what we call the separate meanings of the several words; the terms in a proposition are

modified[1] by each other. Single words are meaningless, and so is any unrelated succession of words. The unit of speech is a proposition. A single word is, or is in effect, a proposition, if other words in relation are implied. The English tourist at a French *table d'hôte* was understood by the waiter to be asking for water when his neighbours thought he was crying "oh!" from distress. It is from the use of a word that we judge of its propositional value. The words "yes" and "no" are propositions, but only when used for assent and dissent; they are used by healthy people interjectionally as well as propositionally. A speechless patient may retain the word "no," and yet have only the interjectional or emotional, not the propositional, use of it; he utters it in various tones as signs of feeling only. He may have a propositional use of it, but yet a use of it short of that healthy people have, being able to reply "no," but not to say "no" when told; a speechless patient may have the full use of it. On the other hand, elaborate oaths, in spite of their propositional structure, are not propositions, for they have not, either in the mind of the utterer or in that of the person to whom they are uttered, any meaning at all; they may be called "dead propositions." The speechless patient may occasionally swear. Indeed he may have a recurring utterance, *e.g.* "Come on to me," which is propositional in structure but not, to him, propositional in use; he utters it on any occasion, or rather on no *occasion*, but every time he tries to speak.

Loss of speech is therefore the loss of power to propositionise. It is not only loss of power to propositionise aloud (to talk), but to propositionise either internally or externally, and it may exist when the patient remains able to utter some few words. We do not mean, by using the popular term power, that the speechless man has lost any "faculty" of speech or propositionising; he has lost those words which serve in speech, the nervous arrangements for them being destroyed. There is no "faculty" or "power" of speech apart from words revived or revivable in propositions, any more than there is a "faculty" of co-ordination of move-

[1] On this matter see an able article in the *Cornhill Magazine*, May 1866. See also Waitz, 'Anthropology' (Collingwood's Translation), p. 241 *et seq.*

ments apart from movements represented in particular ways. We must here say too that besides the use of words in speech there is a service of words which is not speech; hence we do not use the expression that the speechless man has lost words, but that he has lost those words which serve in speech. In brief, Speechlessness does not mean entire Wordlessness.

It is well to insist again that speech and words are psychical terms; words have of course anatomical substrata or bases as all other psychical states have. We must as carefully distinguish betwixt words and their physical bases, as we do betwixt colour and its physical basis; a psychical state is always accompanied by a physical state, but nevertheless the two things have distinct natures. Hence we must not say that the "memory of words" is a *function* of any part of the nervous system, for function is a physiological term (*vide infrà*). Memory or any other psychical state arises *during* not *from*—if "from" implies continuity of a psychical state with a physical state—functioning of nervous arrangements, which functioning is a purely physical thing—a discharge of nervous elements representing some impressions and movements. Hence it is not to be inferred from the rough division we just made of the elements of "talking," and from what was said of their "separation" by disease, that there is anything in common even for reasonable contrast, much less for comparison, betwixt loss of speech (a psychical loss) and immobility of the articulatory muscles from, say disease of the medulla oblongata, as in "bulbar paralysis" (a physical loss). As before said, we must not classify on a mixed method of anatomy, physiology, and psychology, any more than we should classify plants on a mixed natural and empirical method, as exogens, kitchen-herbs, graminaceæ, and shrubs. The things comparable and contrastable in the rough division are (1) the two physical losses: (*a*) loss of function of certain nervous arrangements in the cerebrum, which are not speech (words used in speech), but the anatomical substrata of speech; and (*b*) loss of function of nervous arrangements in the medulla oblongata. (2) The comparison, on the psychical side, fails. There is no psychical loss in disease of the medulla

oblongata to compare with loss of words, as this part of the nervous system, at least as most suppose,[1] has no psychical side; there is nothing psychical to be lost when nervous arrangements in the medulla oblongata are destroyed.

The affections of speech met with are very different in degree and kind, for the simple reason that the exact position of disease in the brain and its gravity differ in different cases; different amounts of nervous arrangements in different positions are destroyed with different rapidity in different persons. There is, then, no single well-defined "entity"—loss of speech or aphasia—and thus, to state the matter for a particular practical purpose, such a question as, "Can an aphasic make a will?" cannot be answered any more than the question, "Will a piece of string reach across this room?" can be answered. The question should be, "Can this or that aphasic person make a will?" Indeed, we have to consider degrees of affection of Language, of which speech is but a part. Admitting the occurrence of numerous degrees of affection of Language, we must make arbitrary divisions for the first part of our inquiry, which is an empirical one.

Let us divide roughly into three degrees: (1) *Defect of Speech.*—The patient has a full vocabulary, but makes mistakes in words, as saying "orange" for "onion," "chair" for "table"; or he uses approximative or quasi-metaphorical expressions, as "Light the fire up there," for "Light the gas." "When the warm water comes the weather will go away," for "When the sun comes out the fog will go away." (2) *Loss of Speech.*—The patient is practically speechless and his pantomime is impaired. (3) *Loss of Language.*—Besides being speechless, he has altogether lost pantomime, and emotional language is deeply involved.

To start with, we take the simplest case, one of *loss of speech*, No. 2 ("complete aphasia"). Cases of defect of speech (1) are far too difficult to begin with, and so, too, are those cases (3) in which there is not only loss of speech,

[1] I, however, believe, as Lewes does, that in so far as we are physically alive, we are psychically alive; that some psychical state attends every condition of activity of every part of the organism. This is, at any rate, a convenient hypothesis in the study of diseases of the nervous system.

but also deep involvement of that least special part of language which we call emotional language. Moreover, we shall deal with a case of permanent speechlessness. I admit that making but three degrees of affection of language, and taking for consideration one kind of frequently occurring case, is an entirely arbitrary proceeding, since there actually occur very numerous degrees of affection of language, many slighter than, and some severer than, that degree (No. 2) we here call one of loss of speech. But, as aforesaid, we must study subjects so complex as this empirically before we study them scientifically; and for the former kind of study we must have what are called "definitions" by type, and state exceptions. This is the plan adopted in every work on the practice of medicine with regard to all diseases. Let us give an example of the twofold study. Empirically or clinically, that is for the art of medicine, we should consider particular cases of epilepsy as each *approaches this or that nosological type* ("le petit mal, le grand mal," &c.). For the science of medicine we should, so far as is possible, consider cases of epilepsy as each is dependent on a "discharging lesion" of this or that part of the cortex cerebri, and thus as it is a *departure from healthy states* of this or that part of the organism. We cannot do the latter fully yet, but the anatomico-physiological researches of Hitzig and Ferrier have marvellously helped us in this way of studying epilepsies, as also have the clinical researches of Broadbent, Charcot, Duret, Carville, and others.[1]

The following are brief and dogmatic statements about a condition which is a common one—the kind of one we call loss of speech, our second degree (No. 2) of Affection of

[1] See Moxon, On the Necessity for a Clinical Nomenclature of Disease, *Guy's Hospital Reports*, vol. xv. In this paper Moxon shows conclusively the necessity of keeping the clinical, or what is above called empirical—not using that term in its popular bad signification—and scientific studies of disease distinct. After reading this paper, my eyes were opened to the confusion which results from mixing the two kinds of study. It is particularly important to have both an empirical arrangement and a scientific classification of cases of Insanity. An example of the former is the much-criticised arrangement of Skae; the scientific classification of cases of insanity, like that of affections of speech, would be regarding them as instances of Dissolution; the Dissolution in insanity begins in the highest and most complex of all cerebral nervous arrangements, the Dissolution

Language. The statements are about two equally important things; (1) of what the patient has lost in Language—his negative condition, and (2) of what he retains of Language—his positive condition. Here, again, is an illustration of a general principle which is exemplified in many if not in all cases of nervous disease, and one of extreme importance when they are scientifically considered as instances of nervous Dissolution. We have already stated the duality of many symptomatic conditions in the remarks on p. 308. Without recognising the two elements in all cases of affections of speech, we shall not be able to classify affections of speech methodically. If we do not recognise the duplex (negative and positive) condition, we cannot possibly trace a relation betwixt Nos. 1, 2, and 3 (p. 314). There can be no basis for comparison betwixt the wrong utterances in No. 1 and the non-utterances in Nos. 2 and 3—betwixt a positive and a negative condition—betwixt speech, however bad, and no speech. There is a negative and a positive condition in each degree; the comparison is of the three degrees of the negative element and the three degrees of the positive element; the negative and positive elements vary inversely. The condition of the patient No. 1, who made such mistakes as saying "chair" for "table" was duplex; (*a*) negatively in not saying "table," and (*b*) positively, in saying "chair" instead; there is in such a case *loss* of some speech, with *retention* of the rest of speech. Hence the term defect of speech applied to such a case is equivocal; it is often used as if the actual utterance was the *direct* result of the disease. The utterance is wrong in that the words of it do not fit the things intended to be indicated; but it is the best speech under the circumstances, and is owing to activity of healthy (except perhaps slightly unstable) nervous elements. The real, the primary, fault is in the nervous elements which do not act, which are destroyed, or are for the time *hors de combat*. If then we compare No 1 with No. 2, we

causing affections of speech in a lower series. The one kind of classification is for diagnosis (for direct "practical purposes"), the other is for increase of knowledge, and is worthless for immediate practical purposes. The fault of some classifications of insanity is that they are mixed, partly empirical and partly scientific.

compare the two negative conditions, the inability to say "table," &c. (the loss of some speech) in No. 1, with the loss of nearly all speech in No. 2, saying the latter is a greater degree of the former, and we compare the two positive conditions, the retention of inferior speech (the wrong utterances) in No. 1, with in No. 2 the retention of certain recurring utterances and with the retention of emotional language, saying the latter is a minor or lower degree of language than the former. Unless we take note of the duplex condition in imperception (delirium and ordinary insanity), we shall not be able to trace a correspondence betwixt it and other nervous diseases. There are necessarily the two opposite conditions in all degrees of mental affections, from the slightest "confusion of thought" to dementia, unless the dementia be total.

The Patient's Negative Condition.

(1) *He does not speak.*—He can, the rule is, utter some jargon, or some word, or some phrase. With rare exceptions, the utterance continues the same in the same patient: we call these Recurring Utterances. The exceptions to the statement that he is speechless are two. (*a*) The recurring utterance may be "yes," or "no," or both. These words are propositions when used for assent or dissent, and they are so used by some patients who are for the rest entirely speechless. (*b*) There are Occasional Utterances. Under excitement the patient may swear: this is not speech, and is not exceptional; the oath means nothing; the patient cannot repeat it, he cannot "say" what he has just "uttered." Sometimes, however, a patient, ordinarily speechless, may get out a phrase appropriate to some simple circumstance, such as "good-bye" when a friend is leaving. This is an exception, but yet only a partial exception; the utterance is not of high speech value;[1] he cannot

[1] What is meant by an utterance of high speech value and by inferior speech will later on be stated more fully than has been just now stated by implication. When we cease dealing with our subject empirically and treat it scientifically, we hope to show that these so-called exceptions come in place under the principle of Dissolution. We may now say that speech of high value, or superior speech, is new speech, not necessarily new words and possibly not new combinations of words; propositions symbolising relations of images new to the speaker, as in care-

"say" it again, cannot repeat it when entreated; it is inferior speech, little higher in value than swearing. However, sometimes a patient, ordinarily speechless, may get out an utterance of high speech value; this is very rare indeed.

(2) *He cannot write;* that is to say, he cannot express himself in writing. This is called Agraphia (William Ogle). It is, I think, only evidence of the loss of speech, and might have been mentioned in the last paragraph. Written words are symbols of symbols. Since he cannot write, we see that the patient is speechless, not only in the popular sense of being unable to talk, but altogether so; he cannot speak internally. There is no fundamental difference betwixt external and internal speech; each is propositionising. If I say "gold is yellow" to myself, or think it, the proposition is the same; the same symbols referring to the same images in the same relation as when I say it aloud. There is a difference, but it is one of degree; psychically "faint" and "vivid," physically "slight" and "strong" nervous discharges. The speechless patient does not write because he has no propositions to write. The speechless man may write in the sense of penmanship; in most cases he can copy writing, and can usually copy print into writing, and very frequently he can sign his name without copy. Moreover he may write in a fashion without copy, making, or we may say drawing, a meaningless succession of letters, very often significantly the simplest letters, pothooks. His handwriting may be a very bad scrawl, for he may have to write with his left hand. His inability to write, in the sense of expressing himself, *is* loss of speech; his ability to make ("to draw") letters, as in copying, &c., shows that his "image series" (the materials of his perception) is not damaged.

Theoretically there is no reason why he should not write music without copy, supposing of course that he could have done that when well; the marks (artificial images) used in noting music, have no relation to words any way used. On this matter I have no observations. Trousseau writes in his

fully describing something novel; by inferior speech is meant utterances like, "Very well," "I don't think so," ready fitted to very simple and common circumstances, the nervous arrangements for them being well organised.

Lecture on Aphasia (*Syd. Soc. Trans.* vol. i. p. 270), "Dr. Lasegue knew a musician who was completely aphasic, and who could neither read nor write, and yet could note down a musical phrase sung in his presence."

(3) In most cases the speechless patient *cannot read at all*, obviously not aloud, but not to himself either, including what he has himself copied. We suppose our patient cannot read. This is not from lack of sight, nor is it from want of perception; his perception is not itself in fault, as we shall see shortly.

(4) His power of making signs is impaired (pantomimic propositionising). We must most carefully distinguish pantomime from gesticulation. Throwing up the arms to signify "higher up," pantomime, differs from throwing up the arms when surprised, gesticulation, as a proposition does from an oath.

So far we have, I think, only got two things, loss of speech (by simple direct evidence, and by the indirect evidence of non-writing and non-reading) and defect of pantomime. There are in some cases of loss of speech other inabilities; the most significant are that a patient cannot put out his tongue when he tries, or execute other movements he is told, when he can move the parts concerned in other ways quite well.

The Patient's Positive Condition.

(1) He can understand what we say or read to him; he remembers tales read to him. This is important, for it proves that, although Speechless, the patient is not Wordless. The hypothesis is that words are in duplicate; and that the nervous arrangements for words used in speech lie chiefly in the left half of the brain; that the nervous arrangements for words used in understanding speech (and in other ways) lie in the right also. Hence our reason for having used such expressions as "words serving in speech;" for there is, we now see, another way in which they serve. When from disease in the left half of the brain speech is lost altogether, the patient understands all we say to him, at least on matters simple to him. Further it is supposed that another use of the words which re-

main is the chief part of that service of words which in health precedes speech; there being an unconscious or subconscious revival of words in relation before that second revival which is speech. Coining a word, we may say that the process of Verbalising is dual; the second "half" of it being speech. It is supposed also that there is an unconscious or subconscious revival of relations of images, before that revival of images in relation which is Perception.

(2) His articulatory organs move apparently well in eating, drinking, swallowing, and also in such utterances as remain always possible to him (recurring utterances), or in those which come out occasionally. Hence his speechlessness is not owing to disease of those centres in the medulla oblongata for immediately moving the articulatory muscles; for in other cases of nervous disease, when these centres are so damaged that the articulatory muscles are so much paralysed that *talking* is impossible, the patient remains able to *speak* (to propositionise) as well as ever; he has internal speech, and can write what he speaks.

The following dicta may be of use to beginners. Using the popular expression "talk," we may say that if a patient does not talk because his brain is diseased, he cannot write (express himself in writing), and can swallow well; if he cannot talk because his tongue, lips, and palate are immovable, he can write well and cannot swallow well.

(3) His vocal organs act apparently well; he may be able to sing.

(4) His emotional language is apparently unaffected. He smiles, laughs, frowns, and varies his voice properly. His recurring utterance comes out now in one tone and now in another, according as he is vexed, glad, &c.; strictly we should say he sings his recurring utterance; variations of voice being rudimentary song (*Spencer*); he may be able to sing in the ordinary meaning of that term. As stated already, he may swear when excited, or get out more innocent interjections, simple or compound (acquired parts of emotional language). Although he may be unable to make any but the simplest signs, he gesticulates apparently as well as ever, and probably he does so more frequently and more copiously than he

used to do. His gesticulation draws attention to his needing something, and his friends guess what it is. His friends often erroneously report their guessing what he wants when his emotional manifestations show that he is needing something, as his expressing what thing it is that he wants.

So far for the negative and positive conditions of Language in our type case of Loss of Speech—No. 2 in Defect of Language.

Words are in themselves meaningless, they are only symbols of things or of "images" of things; they may be said to have meaning "behind them." A proposition symbolises a particular relation of some images.[1]

We must then briefly consider the patient's condition in regard to the images symbolised by words. For although we artificially separate speech and perception, words and images co-operate intimately in most mentation. Moreover, there is a morbid condition in the image series (Imperception), which corresponds to aphasia in the word series. The two should be studied in relation.

The speechless patient's perception (or "recognition," or "thinking" of things) (propositions of images) is unaffected, at any rate as regards simple matters. To give examples: he will point to any object he knew before his illness which we name; he recognises drawings of all objects which he knew before his illness. He continues able to play at cards or dominoes; he recognises handwriting, although he cannot read the words written; he knows poetry from prose, by the different endings of the lines on the right side of the page. One of my patients found out the continuation of a series of papers in a magazine volume, and had the right page ready for her husband when he returned from his work; yet she, since her ill-

[1] The term "image" is used in a psychical sense, as the term "word" is. It does not mean "visual" images only, but covers all mental states which represent things. Thus we speak of auditory images. I believe this is the way in which Taine uses the term image. What is here called "an image" is sometimes spoken of as "a perception." In this article the term perception is used for a *process*, for a "proposition of images," as speech is used for propositions, *i.e.* particular inter-relations of words. The expression "organised image" is used briefly for "image, the *nervous arrangements for which* are organised," correspondingly for "organised word," &c.

ness, could not read a word herself, nor point to a letter, nor could she point to a figure on the clock. There is better and simpler evidence than that just adduced that the image series is unaffected; the foregoing is intended to show that the inability to read is not due to loss of perception nor to non-recognition of letters, &c., as particular marks or drawings, but to loss of speech. Written or printed words cease to be symbols of words used in speech for the simple reason that those words no longer exist to be symbolised; the written or printed words are left as symbols of nothing, as mere odd drawings. The simplest example showing the image series to be undamaged is that the patient finds his way about; this requires preconception, that is "propositions of images" of streets, &c. Moreover, the patient can, if he retains the propositional use of "yes" and "no," or if he has the equivalent pantomimic symbols, intelligently assent or dissent to simple statements, as that "Racehorses are the swiftest horses," showing that he retains organised nervous arrangements for the images of the things "swiftness" and "horse"; this has already been implied when it was asserted that he understands what we say to him, a process requiring not some of his words only, but also some of his "images" of things, of which the words are but symbols.

Such facts as the above are sometimes adduced as showing that the patient's "memory" is unaffected. That expression is misleading, if it implies that there is a general faculty of memory. There is no faculty of memory apart from things being remembered; apart from having, that is, now and again, these or those words, or images, or actions (faintly or vividly). We may say he has not lost the memory of images, or, better, that he has the images actually or potentially; the nervous arrangements being intact and capable of excitation did stimuli come to them; we may say that he has lost the memory of those words which serve in speech. It is better, however, to use the simple expression that he has not lost images, and that he has lost the words used in speech.

These facts as to retention of images are important as regards the writing of speechless patients. The printed or written letters and words are images, but they differ from

the images of objects, in being artificial and arbitrary, in being acquired later; they are acquired after speech and have their meaning only through speech; written words are symbols of symbols of images. The aphasic patient cannot express himself in writing because he cannot speak; but the nervous arrangements for those arbitrary images which are named letters are intact, and thus he can reproduce them as mere drawings, as he can other images, although with more difficulty, they, besides lacking their accustomed stimulus, being less organised. He can copy writing, and he can copy print into writing. When he copies print into writing, obviously he derives the images of letters from his own mind (physically his own organisation). He does not write in the sense of expressing himself, because there are no words reproduced in speech to express. That series of artificial images which makes up the signature of one's name has become almost as fully organised as many ordinary images; hence in many cases the speechless man who can write nothing else without copy can sign his name.

For the perception (or recognition or thinking) of things, at least in simple relations, speech is not necessary, for such thought remains to the speechless man. Words are required for thinking, for most of our thinking at least, but the speechless man is not wordless; there is an automatic and unconscious[1] or subconscious service of words.

It is not of course said that speech is not required for thinking on novel and complex subjects, for ordering images in new and complex relations (*i.e.* to the person concerned),

[1] The expression "*un*conscious reproduction of words," involves the same contradiction as does the expression, "unconscious sensation." Such expressions may be taken to mean that energising of lower, more organised, nervous arrangements, although unattended by any sort of conscious state, is essential for, and leads to, particular energisings of the highest and least organised—the now-organising—nervous arrangements, which last-mentioned energising is attended by consciousness. I, however, think (as Lewes does) that some consciousness or "sensibility" attends energising of all nervous arrangements (I use the term subconscious for slight consciousness). In cases where from disease the highest nervous arrangements are suddenly placed *hors de combat*, as in sudden delirium, the next lower spring into greater activity; and then, what in health was a subordinate subconsciousness, becomes a vivid consciousness, and is also the highest consciousness there then can be.

and thus the process of perception in the speechless, but not wordless, man may be defective in the sense of being inferior from lack of co-operation of speech: it is not itself in fault, it is left unaided.

To understand anything novel and complex said to him, the healthy man speaks it to himself, *e.g.* repeats, often aloud, complex directions of route given to him.

The word "thing" has not been used as merely synonymous with "substance"; nor is it meant that anybody has nervous arrangements for the images of "swiftness" and "horse," but only for images of some swiftly-moving thing or things, and for images of some particular horse or horses.

It may be well here to give a brief recapitulation of some parts of our subject and, also very briefly, an anticipation of what is to come; the latter is given partly as an excuse for having dwelt in the foregoing on some points not commonly considered in such an inquiry as this, and partly to render clearer some matters which were only incidentally referred to.

The division into internal and external speech (see p. 318) is not that just made into the dual service of words. Internal and external speech differ in degree only: such a difference is insignificant in comparison with that betwixt the prior unconscious, or subconscious, and automatic reproduction of words and the sequent conscious and voluntary reproduction of words; the latter alone is speech, either internal or external. Whether I can show that there is this kind of duality or not, it remains certain that our patient retains a service of words, and yet ordinarily uses none in speech. The retention of that service of words which is not a speech use of words, is sometimes spoken of as a retention of "memory of" words, or of "ideas of" words. But as there is no memory or idea of words apart from having words, actually or potentially, it is better to say that the patient retains words serving in other ways than in speech; we should say of his speechlessness, not that he has lost the memory of words, but simply that he has lost those words which serve in speech.

When we consider more fully the duality of the Verbalising process, of which the second "half" is speech, we shall try to show that there is a duality also in the revival of the

images symbolised; that perception is the termination of a stage beginning by the unconscious or subconscious revival of images which are in effect "image symbols"; that we think not only by aid of those symbols, ordinarily so-called (words), but by aid of symbol-images. It is, I think, because speech and perception are preceded by an unconscious or subconscious reproduction of words and images, that we seem to have "faculties" of speech and of perception, as it were, above and independent of the rest of ourselves. We seem to have a memory or ideas[1] of words *and* words; having really the two kinds of service of words. The evidence of disease shows, it is supposed, that the highest mentation arises out of our whole organised states, out of ourselves—that Will, Memory, &c., "come from below," and do not stand autocratically "above," governing the mind; they are simply the now highest, or latest, state of our whole selves. In simple cases of delirium (partial imperception with inferior perception) as when a patient takes his nurse to be his wife, we find, I think, a going down to and a revelation of what would have been when he was sane, the lower and earlier step towards his true recognition or perception of the nurse.

The first step towards his recognition of her when he was sane, would be the unconscious, or subconscious, and automatic reproduction of his, or of one of his, well-organised symbol-images of woman; the one most or much organised in him would be his wife. To say what a thing is is to say what it is like; he would not have known the nurse even as a woman, unless he had already an organised image of at least one woman. The popular notion is, that by a sort of faculty of perception, he would recognise her without a prior stage in which, he being passive, an organised image was roused in him by the mere presence of the nurse; the popular notion almost seems to imply the contradiction that he first sees her, in the sense of recognising her, and then sees her as like his already acquired or organised image

[1] The so-called *idea* of a word, in contradistinction to *the* word, is itself a word subconsciously revived, or revivable, before the conscious revival or revivability of the same word, which latter, in contradistinction to the so-called *idea of* a word, is the so-called *word itself—the* word.

of some woman. We seem to ourselves to Perceive, as also to Will and to Remember, without prior stages, because these prior stages are unconscious or subconscious. It seems to me that in delirium the patient is reduced to conditions which are revelations of, or of parts of, the lower earlier and prior stages; the lower or earlier stages are then conscious. They are the *then* highest or latest conscious states. When the patient becomes delirious, he takes the nurse to be his wife. More or fewer of the highest nervous arrangements being then exhausted, the final stage is not possible; there is only the first stage; the reproduction of his well organised symbol-image is all there is, and that is all the nurse can be to him; she is, to him, his wife. The symbol-image is then vividly reproduced because the centres next lower than those exhausted are in abnormally great activity (note, that there are two conditions, one negative and the other positive). There is a deepening of consciousness in the sense of going down to lower earlier and more organised states, which in health are mostly unconscious or subconscious, and precede higher conscious states; in other words with loss or defect of object consciousness, even in sleep with dreaming, there is increasing subject consciousness; on the physical side, increasing energising of those lower centres which are in the day time more slightly energising during that unbroken subconscious "dreaming," from which the serial states, constituting our latest or highest object consciousness, are the continual "awakenings."

It is supposed that the well organised images spoken of—in effect arbitrary images, symbol-images, those which *become* vivid and are "uppermost" in delirium, and then cease to be mere symbols—constitute what seems to be a "general notion" or "abstract idea" of such things as "horse," "swiftness," &c.; their particularity (that they are only images of some horse or horses, of some swift moving thing or things) not appearing, because they are unconscious or subconscious; they served once as images of particular things, and at length as symbol-images of a class of images of things, as well as images of the particular things.

At page 319 we spoke of the right half of the brain as

being the part during the activity of which the most nearly unconscious and most automatic service of words begins, and of the left as the half during activity of which there is that sequent verbal action which is Speech. The division is too abrupt; some speech—voluntary use of words—is, as we have seen, when alluding to Occasional Utterances, possible to the man who is rendered practically speechless by disease in the left half. Again, from disease of the right half, there is not loss of that most automatic service of words which enables us to understand speech. The thing which it is important to show is, that mentation is dual, and that physically the unit of function of the nervous system is double the unit of composition; not that one half of the brain is "automatic" and the other "voluntary."

Having now spoken of the kind of case we shall consider, and having added remarks, with the endeavour to show how the several symptoms—negative and positive—are related one to another, we shall be able to give reasons for excluding other kinds of cases of speechlessness.

We are not concerned with cases of all persons who do not speak. We shall not, for example, deal with those untrained deaf-mutes who never had speech, but with the cases of those persons only who have had it, and lost it by disease. The condition of an untrained deaf-mute is in very little comparable with that of our arbitrarily-taken case of loss of speech. The deaf-mute's brain is not diseased, but, because he is deaf, it is uneducated (or in anatomical and physiological phraseology undeveloped) so as to serve in speech. Our speechless patient is not deaf. Part of our speechless patient's brain is destroyed; he has *lost* nervous arrangements which had been trained in speech. Moreover, our speechless man retains a service of words which is not speech; untrained deaf-mutes have no words at all. Further, the untrained deaf-mute has his natural system of signs, which to him is of speech value so far as it goes. He will think by aid of these symbols as we do by aid of words.[1] Our speechless patient is defective even in such slight

[1] We must not confound the finger-talk with the "natural" system of signs. They are essentially different. No one supposes that words are essential for

pantomime as we may reasonably suppose to have been easy to him before his illness. The deaf-mute may have acquired for talking and thinking the common arbitrary system of deaf-mute signs (finger-talk), or he may have been taught by the new method to speak as we do, and thus have ceased to be mute. But when not taught to speak, he is not in a condition even roughly comparable with that of a man who has *lost* speech. No doubt by disease of some part of his brain the deaf-mute might lose his natural system of signs, which are of some speech value to him, but he could not lose speech, having never had it. Much more like our speechless patient's condition is that of the little child which has been taught to understand speech, and has not yet spoken.

There is another set of cases of so-called loss of speech, which we shall not consider as real loss of speech. I prefer to say that these patients *do* not speak: cases of some persons are meant, who do not talk and yet write perfectly. This may seem to be an arbitrary exclusion. There is in most of these cases an association of symptoms, which never arises from any local disease of any part of the nervous system; the so-called association is a mere jumble of symptoms. Let us state the facts. The patients are nearly always boys or unmarried women. The bearing of this is obvious. The so-called loss of speech is a total non-utterance, whereas it is an excessively rare thing for a patient who does not speak, because his brain is locally diseased, to have no utterance whatever; I do not remember seeing one such case in which there was not some utterance (recurring utterance) a few days or a few weeks after the onset of the illness; the absolute pseudo-speechlessness may remain for months. They cannot be mute from paralysis of the articulatory muscle, because they swallow well. Frequently there is loss of voice also—they get out no sounds except, perhaps, grunts, &c.—and yet they cough ringingly and breathe without hoarseness or stridor; there is no evidence of laryngeal disease. Now loss of voice never occurs with loss of speech from local disease of one side of the

thought, but only that some symbols are essential for conceptual thought, although it may be that people with "natural" symbols do not reach that higher degree of abstract thinking which people do who have words.

brain. No disease of the larynx would cause loss of speech or loss of articulation. The patients often "lose" their speech after calamity or worry. In these cases there is no hemiplegia and no other one-sided condition from first to last. They often, after months of not-speaking, recover absolutely and immediately after some treatment which can have no therapeutical effect, *e.g.* a liniment rubbed on the back, a single faradaic stimulation of the vocal cords or of the neck. Dr. Wilks has reported a case of "cure" of a girl who had not spoken for months; she had also "lost" the use of her legs. Knowing well what was the general nature of the case, Dr. Wilks, by speaking kindly to her, and giving her an excuse for recovery in the application of faradisation, got her well in a fortnight. Sometimes the so-called speechless patient speaks inadvertently when suddenly asked a question, and then goes on talking; is well again. Sometimes speech is surprised out of her. Thus a woman, whose case is recorded by Durham, when told to cry "ah!" when the spatula was holding down her tongue, pushed his hand away, saying, "How can I, with that thing in my mouth?" She then said, "Oh! I have spoken." She was "cured." I believe that patients, "speechless" as described, might be "cured" by faradisation of the vocal cords, or by a thunderstorm, or by quack medicines or appliances, or by mesmerism, or by wearing a charm, or—not speaking flippantly—by being "prayed over."

Sometimes these cases are spoken of as cases of "emotional aphasia"—the speechlessness is said to be "caused by" emotional excitement, because it often comes on *after* emotional disturbance.

I submit that the facts that the patients do not talk and *do* write and *do* swallow are enough to show that there is no disease at all, in any sense except that the patients are hysterical (which is saying nothing explanatory), or that they are pretending. There can be no *local* disease, at any rate.

These cases are spoken of at length, although they are excluded, because they are sometimes adduced as instances of aphasia, or loss of speech proper, with ability to write remaining. I confess that were I brought face to face with a man whom I believed to *have* local disease of his brain, who did not

talk, and yet wrote well, I should conclude that he did *speak* internally although he could not talk. To say that *he* cannot speak and yet can express himself in writing is equivalent, I think, to saying *he* cannot speak and yet *he* can speak.

[A further instalment of this article will appear in some future number of BRAIN.]

METALLOSCOPY AND METALLOTHERAPY.

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THE treatment of disease by the external application of metals has been practised from the earliest ages. The ancients wore rings and amulets which, combined with supplications and sacrifices to the gods, performed marvellous cures. Paracelsus is reported to have placed metallic substances on the skin to relieve internal complaints, and Pomme, two centuries later, became celebrated for his treatment of nervous disorders by means of copper baths. The discovery during the eighteenth century of galvanism and electricity gave a fresh stimulus to researches on this subject, and the enthusiasm created by the knowledge of these powerful agents added a fresh impulse to the study of metallotherapy; galvanic and electric piles, batteries, and the application of simple metals were energetically employed as curative agents. Perkins, at the commencement of this century, used needles of different metals, which he thrust into the body with the view of establishing electric currents. Twenty years later, when electricity began to be applied scientifically to medicine, various methods of external treatment by metallic substances were employed. Among others Raspail used plates of zinc and copper, Goldberger introduced galvanic chains, Sage applied powders of copper and steel, Recamier suggested poultices of copper and zinc, Pulvermacher constructed chains of zinc and copper, and there arose a host of other contrivances for the purpose of influencing abnormal states by means of external metallic applications.

M. Burq, in 1848, was the first to make a scientific investigation into, and to form any definite system on, the external application of metals to the treatment of disease, and in a remarkable work, he describes his extensive observations and experiments on the subject.[1] He ascertained that workers in copper were exempt from cholera during many of the severest epidemics of Paris, and he subsequently found that the external and internal administration of that metal relieved the symptoms of others when attacked with the disease in question.[2] He then maintained that chlorosis was cured by the external application of various metallic substances, which he accounts for by considering that affection not a disorder by itself, but only the symptom of a nervous disease over which this procedure has a beneficial influence.[3]

He subsequently applied his system to a variety of other affections, but met with the greatest success in the treatment of hysteria, the most troublesome symptoms of which were often relieved or cured.

Burq's method of procedure was first by means of small discs to ascertain the metal which had some influence on the patient, and then to envelop him in a sort of armour of the same substance. This cuirass was kept on for days, weeks, or months, until the beneficial changes were considered complete. The works of M. Burq during the last thirty years contain so many and remarkable cures by what seems so simple a process, that it is singular that the profession at large have apparently so little benefited by his instructions. Burqism, however, like Braidism, seems to be a doctrine which is more efficacious in the hands of its author than in those of others, and the results, if genuine, may possibly be due to the personal influence of an individual rather than to the exactness of a system. Hence both have hitherto been abandoned by the medical profession in the practical treatment of disease. They are closely related, Burq himself expressing great sympathy with mesmerism, and he admits that by means of metals he can

[1] 'Métallothérapie,' par Dr. V. Burq. Paris, 1853.
[2] 'Du Cuivre contre le Choléra,' par Dr. V. Burq. Paris, 1867.
[3] 'L'Etude de la traitement de la Chlorose,' par Dr. V. Burq. Paris, 1852.

prognose those likely to be sensitive subjects in the operations of the Hypnotiser.

Professor Charcot, of Paris, has recently revived the system of Burq in the treatment of "Grave Hysteria," and in a recent number of the *Lancet* his views are expounded.[1] His observations have since been apparently confirmed by Dr. Henry Thompson,[2] Mr. Horrocks,[3] in the wards of Dr. Wilks[4] and others, both at home and abroad.

I have repeated all the experiments and observations of these authorities in several cases of anæsthesia and analgesia which have recently been under my care, and although brevity compels me to omit details, I venture to say a few words on the results obtained, and will use the writings of the above authors as a text for my remarks.

1. It is stated that in certain cases of anæsthesia and analgesia the application of various metals is followed by a return of sensibility to touch and pain. This I have ascertained to be a fact. In a case at present under observation, where there is complete hemianæsthesia, including the special senses of the right side, a limb treated after the process of Burq and Charcot is often entirely restored to its natural function. This restoration is usually temporary, but is sometimes permanent. Occasionally the effect extends over the whole body, but is more often confined to the limb or neighbourhood of the applied metal.

It is also asserted that each individual is influenced by one particular metal, while others fail to have a beneficial action. As far as my experience has gone it is not in accord with this statement: I have found, after repeated and extended trials, that it apparently makes little difference what substance is employed, the result being in all cases the same. For example, in one patient I tested a variety of metals without the slightest effect. Finally, on employing zinc, local sensibility was completely restored for some hours. Next day zinc was again used with similar results, and so on. Subsequently, on applying the metals which had formerly failed, the same phenomena ensued,

[1] *Lancet*, January 19th, 1878.
[2] *British Medical Journal*, November 30th, 1877.
[3] *Guy's Hospital Gazette*, April 1878.
[4] *British Medical Journal*, July 19th, 1878.

and sometimes when zinc was tested no change whatever took place. In short, in this case the application of metals usually induced changes in sensation, but there was no constancy as to the effects produced by any particular one of them; that which caused a return of sensibility yesterday did not to-day, and that which failed to-day might succeed to-morrow.

Finally, this question arose. Are the phenomena induced really due to any special virtue in the metal, as is alleged? To solve this problem I have applied discs of wood, and have produced thereby all the phenomena described as following the application of the metals. In a case at present in the Westminster Hospital, of a woman with complete hemianæsthesia and hemianalgesia from which she had suffered for upwards of a year, after testing a number of metals with varying success, I applied two wooden buttons to her arm. From complete insensibility to pain and touch in half an hour the arm was normal to both, and has remained so ever since, that is for the last three months, the other parts of the body continuing anæsthetic and analgesic as before. This observation with the wooden discs has been frequently repeated, and the results have been, in the writer's experience, as certain and constant as from any single metal.

2. It is stated that the muscular force, as measured by the dynamometer, is increased by the application of metals in anæsthetic cases. I have also found this statement in a certain degree to be correct. There can be no doubt that the arm and hand when sensitive to touch can exert a greater force than when they are anæsthetic. But whether due to any special metallic virtues is more than questionable, as the same results followed the treatment by wooden discs. In short, so far as my observations extend, I have found that a relation exists rather between the power and the sensation, from whatever cause modified, than between the power and the metal employed.

3. It is asserted that external metallic applications exert an influence on the circulation from the fact that when anæsthetic skin is pricked no blood flows from the punctures; but that after metals have been applied and the sensibility restored, this takes place. To a certain extent these observations are correct,

and I have repeatedly confirmed them. I have seen frequently that a puncture of anæsthetic skin by a needle did not bleed, while fresh pricks after the skin became sensitive were followed by bleeding points. But I have never seen the *same* punctures which did not bleed at first afterwards do so when the metal was applied. That these changes are due to the action of the metals *per se* on the circulation is also doubtful, as the same phenomena followed the application of the wooden discs.

The following observation, made by myself, may have some bearing on this subject. In a normal condition of the skin, if a needle be deliberately thrust through its thickness, taking care to avoid a vein, on withdrawal of the instrument no blood follows, as a rule. If, however, the skin be lightly pricked, small spots of blood issue from each puncture. If the same operations be made on anæsthetic skin, precisely similar phenomena ensue. In short, the nature of the wound inflicted by the needle seems alone to determine if blood shall follow, and this whether the skin be anæsthetic or not. This may serve to explain the circulatory changes supposed to be produced by the metals. When skin is anæsthetic the operator is not particular, he thrusts the needle deeply, and no blood in consequence appears. When sensibility is restored he is obliged to be more careful, and gently touches with the point of his instrument the sensitive integument, and, as nearly always occurs, blood is seen at several points. After the use of the metals I have frequently observed redness of the skin, but have always considered that this was due to the previous pricking and pinching necessary to test its anæsthetic condition. It must, however, be admitted that it is highly probable that the return of sensibility to a part indicates a change in its capillary circulation, and that sensitive portions of the skin are more likely to be vascular than those which are anæsthetic.

4. The phenomenon called by Burq "l'anesthésie de retour," I have occasionally seen as described by Charcot, but by no means constantly. He says that when the metal has been applied to anæsthetic skin, and sensibility restored, and if the treatment be continued, at a given moment the anæs-

thesia returns more persistently than before. I have never observed this sudden change. In my experience, the sensibility having been restored, either by metals or discs of wood, it remains an hour or more, and then gradually is lost. In some cases the application having been maintained for twenty-four hours, the anæsthesia was as great as before; in others, however, the sensibility was markedly increased. Charcot then says, if the metals are removed at the moment sensibility is restored, the effects are often permanent, and spread over the remainder of the body. This I have found to take place; but on the other hand, the same circumstances existing, the anæsthesia has often returned in a very short time, and when it remained has been limited to the spot or limb originally restored. In the case before cited, at present under observation, the right arm from absolute anæsthesia was restored to normal sensibility after the application of two discs of wood for an hour. The arm has remained in the same condition for three months, the rest of the body continuing anæsthetic.

5. The "*phénomène du transfert*" discovered by Charcot, he describes as follows:—Having applied a metal to an anæsthetic arm, and restored sensibility to a portion of it, the exactly corresponding part of the other, and hitherto normal, limb is found to be anæsthetic, and as this latter returns to its normal condition the former resumes its previous state. I myself have never observed such an exact displacement of sensibility. It has been noticed that occasionally, when the skin on one side has been restored to feeling of touch and pain, the corresponding portion of the other side has become less sensitive than before; but frequently no such phenomenon has taken place, one limb being changed, the other remaining in its usual condition.

6. The achromatopsy observed by Charcot, in which the patient loses the notion of colours in a certain mathematical regularity, and after the application of metals regains the knowledge of them in the same order, I have had no opportunity of definitely testing. In one case of hemianæsthesia on admission the patient could not, with her right eye, distinguish any colours; all seemed alike grey, some appearing

darker than others. A few days afterwards she said that blue was green, green was blue, light blue was grey, but failed even to attempt to name any others. A week subsequently she told all the colours correctly, and has done so ever since. This patient, up to that time, had received no treatment whatever.

7. Metalloscopy being the search for the metal which affects the patient, metallotherapy consists in employing the same for the treatment of disease, either by administering a solution of it internally, or by applying plates of it externally. From the foregoing remarks it is obvious that I can have had little opportunity of testing this, as I have never yet found a patient who was affected by one particular metal, and by it alone. In a case in which discs of zinc removed the anæsthesia, valerianate of zinc was taken for a month without any apparent effect; and in another case of anæsthesia of the leg, which was modified by the application of two pieces of wood, I, in joke, gave her frequent dram doses of infusion of quassia, and the patient declared herself well in a week. In a third case, where a small disc of zinc restored sensibility, a large plate of the same failed to do so the following day. Many remarkable cases, however, described by Burq, Charcot, Thompson, and others, may deserve consideration from the reputation of the observers.

In reviewing the whole question, one point may be admitted, namely, the genuine nature of these remarkable phenomena; and I can personally guarantee that in my own observations every possible precaution was taken to detect malingering or unconscious deception, and to test the real accuracy of the results. The patients were all carefully blindfolded during the entire period of the experiment; not a word was spoken concerning the nature of the observation; no suggestive ideas were given, and no leading questions asked.

The practical problem to be solved appears to me to be this: Are the beneficial changes due to some special effect, electrical or otherwise, derived from a particular metal, or do they result from the influence its application exercises on the mind, which in its turn directly affects the body? Precisely the same question was asked some years ago when "electro-biology"

startled the public mind by its marvellous revelations, and was conclusively answered by Braid and others. From the earliest ages, cures and miracles have been performed by very much similar processes, many of which were doubtless genuine, but time and scientific investigation have always proved them to be subjective phenomena. My own observations certainly seem to support this view of the question; at the same time, my experience is small, for cases of "grave hysteria" are rare in Anglo-Saxon England, as compared to France with her Latin and Celtic races. Even Charcot declines to give a decisive reply to this question, but contents himself by drawing the conclusion "that the question is deserving of careful attention." He very properly points out that in such investigations one "should keep aloof as much as possible, both from the arbitrary scepticism which too often arises from pedantic ignorance, and from that naïve credulousness which characterises the charcoal burner's faith. It is between these two reefs, which are equally dangerous, that the observer must know how to steer."

Without wishing to give a hasty or decisive opinion on the matter, or to deny that subsequently another explanation may prove satisfactory, it must be admitted the above observations tend to show that the beneficial changes resulting from the application of metals to the skin are due rather to the mental effects they produce than to any special virtues emanating from the metals themselves. This is further supported by the following considerations:—

1st. These phenomena occur for the most part in women, and in persons of a hysterical temperament, who are proverbially prone to rapid and sudden changes readily induced by emotion or expectant attention.

2nd. None of the effects resulting from the application of metals are inconsistent with what is well recognised as the influence the mind possesses over the body.

3rd. The symptoms of anæsthesia and analgesia are so changeable and unstable as to render it difficult to say positively when they are directly affected by remedies. I have, for example, at present under my care in the wards of the Westminster Hospital a girl who frequently for hours

together has complete anæsthesia and analgesia of the right thigh. At other times her skin is perfectly normal, and this without any treatment or apparent cause. We have also no accurate physical means of measuring sensibility, and are for the most part dependent on the statement of the patient.

4th. The facts that (*a*) the action of the metals has been in the hands of the writer uncertain and inconstant, many of these inducing similar effects at different times, and any particular one causing diverse actions on separate occasions; (*b*) that no metal is apparently specially suitable for any given patient, for that which fails at one time may succeed at another; and (*c*) that no metal at all is required, discs of wood having produced all the alleged phenomena—would seem to indicate that the effects of metallotherapy are of mental and not of physical origin; and that, however highly endorsed, we should pause before accepting too readily a theory which appears to be only a revival of a long-exploded doctrine.

A STUDY OF FOUR HUNDRED AND FIFTEEN CASES OF TETANUS.

BY DAVID W. YANDELL, M.D.,

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SOME years back I collected from such medical journals, domestic and foreign, as were within my reach reports of three hundred and eighty-five cases of tetanus. To these were added thirty unpublished cases gathered from practitioners in this city, making a total of four hundred and fifteen cases. The work of tabulating and examining these was kindly done by my friend and colleague Prof. Cowling. The results obtained I propose to summarise here.

Of the total, two hundred and thirty-three cases recovered, one hundred and eighty-two died. It is apparent that for purposes of comparison such results are clearly worthless; but they serve to show that statistics on this point are as imperfect now as when Mr. Curling published his tables in 1837. This obviously arises, I think, from the practice of reporting recoveries only, deaths not being considered a matter of interest. In confirmation of this opinion, I have to state that of the thirty cases now published for the first time, 90 per cent. died. In the three recoveries, one went without treatment, except systematic nourishment; the second was brought through by a "Thompsonian" with steam and lobelia; while the third, having for its only symptom but a slight trismus, which disappeared after a little hypodermic atropia, cannot fairly be regarded as tetanus. Further, the character of the practitioners who treated these cases is an ample guarantee that everything was done which patience and skill

could suggest, the frightful mortality belonging simply to the history of the affection.

But though the tables before me are worthless as to aggregate deaths and recoveries, and are perhaps of no real value as denoting the comparative efficacy of different remedies, yet I think that in the lessons they teach it will be found that in more than one important particular, at least, they serve to throw light on tetanus, the date of its invasion and the duration of the tetanic symptoms, and the influence of these on the general result.

Sex.—This is stated in but 375 cases, 80 per cent. of these were males. The greater exposure of men to the exciting causes of tetanus is usually regarded as sufficient to explain this disparity, yet in 34 cases of idiopathic tetanus 70 per cent. were males. Of males, 44 per cent. recovered; of females, 58 per cent., or, excluding puerperal tetanus, 63 per cent. recovered. It will be observed that these results quite approximate those furnished by Mr. Curling, while they differ from those stated by Mr. Poland,[1] who finds a much greater preponderance of cases in males and of deaths in females.

Age.—The first decade of life furnished 7 per cent. of all the cases. Of these, 62 per cent. died. If prognosis in tetanus be influenced by age, it is less favourable at this period than between ten and twenty years, where the cases rose to 20 per cent., while the mortality fell to 38 per cent. The death-rate could not be made out in the remaining decades, owing to the large number of cases which were reported simply as adults. Fifteen cases are recorded as occurring in individuals over fifty years; and one case in a man aged eighty-nine is reported.

If this very striking difference in the death-rate of so large a number of cases is not a mere coincidence, it would seem very clearly to indicate that while tetanus is comparatively rare, it is also peculiarly fatal in persons under ten years of age; and if to this list the cases of trismus nascentium ending fatally be added, the percentage of deaths would be enormously augmented.

[1] Holmes's 'System of Surgery.'

The tables furnish nothing new as to the character of the wounds which are succeeded by tetanus. Wounds of the extremities and punctured wounds still cut the largest figure in this class of injuries. The popular belief that injuries of the foot are more liable than those of other parts to be followed by tetanus is quite confirmed as to punctured wounds in this situation—the large majority being inflicted by nails run into the foot. Yet the cases as a whole give but eighty-eight wounds of the foot against ninety injuries of the hand. There were twenty-eight instances of wounds of the upper extremities, exclusive of the hand, and sixty-seven in the lower extremities, exclusive of the foot. Wounds about the head are credited with twenty-eight cases; injuries of the trunk with four, chancre with one, abrasion of the cornea with one, while ten followed abortion. Neither the situation nor character of the injury seemed to exercise any influence on the prognosis of the disease. Forty per cent. of deaths followed wounds of the hand and 44 per cent. wounds of the foot. The death-rate in puerperal tetanus reached 90 per cent.

Date of Invasion.—A much more important point than either of the foregoing is the time at which tetanic symptoms appear. The disease supervened within two weeks after the injury in one hundred and ninety-six cases. Of these, 62·5 per cent. died. From the fourteenth to the twenty-first day, 17 per cent. of eighty-one cases were fatal. From the twenty-first till the forty-fourth day there were seventeen cases, of which 17 per cent. died. In cases occurring within seventy-two hours after injury, recoveries exceeded deaths as 14 to 11. In such as occurred between the fourth and the ninth day, deaths exceeded recoveries as 73 to 33—69 per cent. In cases beginning on the thirteenth day, deaths stood to recoveries as 7 to 3; but where tetanic symptoms were delayed till the fourteenth day, recoveries were notably in excess of deaths—23 per cent.

It is a source of regret that in more than one-fourth of all the cases the date of invasion is not given; but it has been recorded in a sufficient number, I think, to show both the difference between acute and chronic tetanus—the difference between the percentages themselves—and finally, how these

differ from the standard of 46 per cent. of total deaths. It is true that the date of invasion is not the only element of dissimilarity between acute and chronic tetanus. But it is an important one; the degree of tetanic symptoms, the muscles involved, pulse, temperature, &c., are others which should not be lost sight of, but which these tables fail to show with a fulness sufficient to warrant even an approximative conclusion.

Duration of Symptoms.—The tables confirm in the main the well-known aphorism of Hippocrates, that tetanus, when fatal, is nearly always so within the first four days after attack. Sir Thomas Watson suggested that the period be extended to nine days. The first notable diminution in death ratio, as shown in the tables, occurred in those cases which had survived fourteen days. From that period the recoveries were progressively in excess of deaths. To illustrate: in tetanus continuing four days, eighteen cases give 50 per cent. of deaths; continuing nine days, there were one hundred and thirty-one deaths to thirty-nine recoveries, while in one hundred and five cases continuing from the ninth to the twenty-third day, there were eighty-two recoveries to twenty-three deaths. In seventy-four cases which survived beyond the twenty-third day but three died, death occurring in each on the twenty-eighth day. The largest number of deaths, thirty-one, fell on the second day. On the fourth day there there were twenty-one deaths. There is a marked falling off in deaths on the fifth day, when there were but eleven deaths; from this time the percentage of deaths steadily diminished. The longest duration of symptoms is two hundred and forty days; the shortest, ending in recovery, is alluded to in the beginning of this article; the shortest in death, is in a few minutes. Nine cases lasted forty-two days; eighteen cases lasted thirty days. Between these two periods we find nineteen cases. Sixty and ninety days are also represented.

The Pulse and Temperature.

These have been recorded in too small a number of instances to admit of any deductions.

Dr. Radcliffe (Reynolds's 'System of Medicine') has stated

that there is an absence of fever in tetanus, and explains the heat and frequency of pulse, when they occur, by the dyspnœa, which urges the skin to take on compensative action. In three cases in which the temperature was recorded, and published now for the first time, the mercury rose to 106°–107°. They were all fatal. In two other cases, also fatal, observed by the writer, the pulse was one hundred and twenty from the beginning of the attack.

Treatment.

This is a matter which has been so varied and so complicated in different cases as to set at defiance all efforts at anything approaching accurate tabulation. The bare enumeration of the remedies used would more than fill one of these pages. Selecting the fifty leading agents to which recoveries are credited, it will be found that in a very small number of cases indeed has the treatment been trusted to a single agent alone. In the large majority of instances very varied combinations of the most powerful along with the least active agents in the Pharmacopœia were made, whereby it was rendered quite impossible to decide what part was played by either in the cure. In the following case, for instance, which I may add is but a type of many such found in the tables, the cure is credited to the subcarbonate of iron:—V. S. ʒ xvj.; electuary of subcarb. iron and turpentine; emp. vesic. over epigastrium, enemas of turpentine, friction with camphor liniment, tonics. Certainly no one can believe that either of the agents mentioned exerted the slightest influence over the course of the disease.

In order to determine as far as possible the relative value of the various plans of treatment practised, everything which seemed to possess any real power has been carefully noted; also the percentage of recoveries under each; the number of recoveries occurring in cases where tetanus followed within nine days after the injury, the number of recoveries where the disease continued less than fourteen days; and finally the mortality in cases where the date of invasion was over nine days after injury, and where the disease continued beyond fourteen days.

The importance of viewing the management of tetanus from this standpoint will be found in the fact that a careful study of these tables goes, I think, to show that the *results* of treatment by the different remedies are so nearly the same *that no one agent can justly be said to possess a decided superiority over any other.*

The mere number of hours or days which intervene between an injury and a subsequent attack of tetanus, is, I am aware, but one of the characteristics which distinguish acute from chronic tetanus; but it is, perhaps, the very heaviest factor in any attempt to estimate the effect of treatment and the comparative value of the remedies used. I think that without it no correct opinion can be formed as to the part which any individual agent, or which treatment, as a whole, plays in the management of the disease.

In order to economise space rather than because it adequately portrays the several features whereby acute is known from chronic tetanus, I shall in what follows designate that form of disease which supervenes within nine days after the injury as acute, and that which occurs subsequently as chronic tetanus.

Aconite.—Fourteen cases; all chronic; 8 per cent. recovered; none recovered before fourteen days.

Amputation.—60 per cent. of seventeen cases recovered; four acute; four in less than fourteen days. Three chronic cases died, and one after fourteen days.

Bleeding.—55 per cent. of fifty-eight cases recovered: nine acute, ten before the fourteenth day. Seven chronic cases died, and two after fourteen days.

Calabar Bean.—39 per cent. of recoveries in thirty-nine cases. One recovery only occurred in the acute form of the disease. In five others recovery took place before the expiration of fourteen days. *Per contra* ten cases of chronic tetanus proved fatal.

Chloroform relieved 70 per cent. of thirty-five cases, nine of which were acute, and eight recovered before fourteen days. Three chronic cases died, and two after symptoms lasted fourteen days.

Cold Effusion.—73 per cent. of eleven cases recovered; three acute; three before fourteen days. Two chronic cases died.

Division of nerve relieved 75 per cent. of three cases: one acute; one before the fourteenth day. One chronic case died.

Ether.—6 per cent. of fifteen cases recovered: five acute; seven inside of fourteen days. One chronic case died.

Indian Hemp.—Used in twenty-five cases, with 64 per cent. of recoveries, of which three cases were acute, and six recovered before the symptoms lasted fourteen days. One chronic case died, and one after fourteen days.

Ice-Bags.—77 per cent. of nine cases recovered: one acute; two in less than fourteen days.

Mercury.—57 per cent. of seventy-five cases got well: eight acute; twelve before fourteen days. Seventeen chronic cases were lost, and two after fourteen days.

Opium.—57 per cent. of 165 cases recovered: twenty-two acute; twenty-nine before fourteen days. Twenty-six chronic cases were lost, and four after the disease had continued fourteen days.

Purgatives.—66 per cent. of seventy-four cases recovered: thirteen acute; twelve before fourteen days. Ten chronic cases died, and three after fourteen days.

Quinine.—73 per cent. of fifteen cases recovered: one acute; three before fourteen days. Three chronic cases ended fatally, and one after fourteen days' duration.

Stimulants.—8 per cent. of thirty-three cases recovered: four acute; six within fourteen days. Six chronic cases died, and three after fourteen days.

Tobacco relieved 50 per cent. of forty-one cases; six acute; six before fourteen days of the disease. Four chronic cases died, and one after fourteen days.

Turpentine relieved 70 per cent. of sixteen cases: six acute; four before fourteen days. Five chronic cases died, and two after fourteen days.

I think these facts certainly show, as far as they go, that we are not improving in our treatment of tetanus. Chloroform ranks in these tables as the most efficient agent yet known in acute tetanus; while even mercury and venesection, which belong to a past age, excel the Calabar bean, of which so much

was expected, not only in general results, but in power of control over the disease in its acute form. An astonishing fact is that in the long list of remedies there are but three—musk, woorari, and warm baths—where the percentage of recoveries was less than by the Calabar bean; and even here the first and last of this trio effected an equal number of cures of acute tetanus. It is but just to the friends of the bean to say that these tables were prepared when the effects and doses of this drug were not so well understood as at present; yet in nine cases of tetanus seen by the writer since that time, and all treated by the bean and chloral hydrate, the percentages remain quite the same. The conclusions which a careful review of the tables would seem to warrant may be summarised as follows:—

I. That traumatic tetanus is most fatal during the first decade of life.

II. That it usually supervenes between four and nine days after the injury.

III. That the largest number of recoveries are found in cases in which the disease occurred after the lapse of nine days from the injury.

IV. When tetanus continues fourteen days, recovery is the rule and death the exception, *apparently independent of the treatment.*

V. Tetanus arising during the puerperal state is the most fatal form of the disease.

VI. Chloroform has up to this time yielded the largest percentage of cures in acute tetanus.

VII. The true test of a remedy for tetanus is its influence on the history of the disease: (*a*) Does it cure cases in which the disease occurred prior to the ninth day after the injury? (*b*) Does it fail in cases whose duration exceeds fourteen days?

VIII. Tried by these tests, no agent has yet established its claims as a true remedy for tetanus.

APPLICATION OF FREEZING METHODS TO THE MICROSCOPIC EXAMINATION OF THE BRAIN.

BY BEVAN LEWIS, F.R.M.S.,

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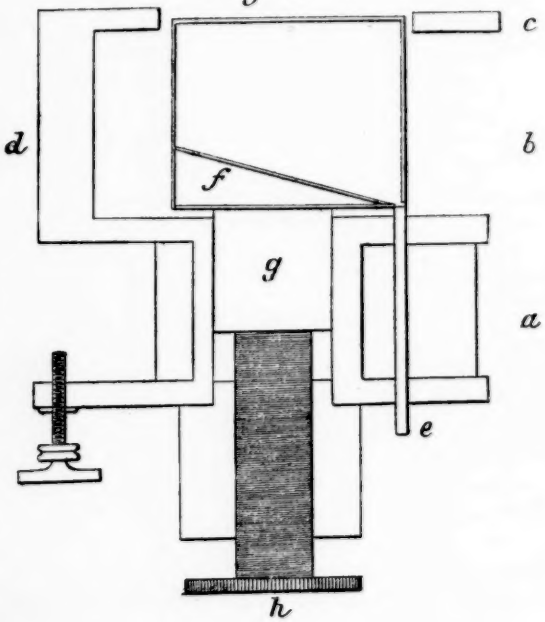
THE adoption of freezing methods for the purpose of obtaining sections of the tissues in a perfectly fresh state has proved an invaluable boon to histologists. Of all processes that of freezing must be regarded as the most elegant and trustworthy for this purpose; and great as has been the success achieved by the use of hardening reagents, such as alcohol, picric acid, osmic acid, and chrome salts, it must be admitted that the most reliable information can only be obtained by examination of tissues in the fresh state. Such preparations form the most trustworthy standards of comparison with which we should habitually contrast results obtained by the employment of chemical reagents. We can by no means afford to dispense with the chrome-hardening processes in studying the central nervous system; it is an old and valued friend, reliable in certain spheres of action, yet often misapplied—made use of for purposes to which it was never adapted, issuing in results of the most confusing character, and receiving from critics that ill-merited opprobrium with which the thoughtless microscopist should alone be credited. In the present state of microscopic science it appears to me scarcely justifiable for an original investigator to limit himself to any single method in the examination of the elements of the nervous system, nor can any of us hope to possess authoritative claim to hearing for results obtained by the exclusive employment of either the hardening or the fresh methods of examination. Nowhere

Fig. 1.



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is more attention paid to the truth of what I here assert than in that unrivalled work of Ranvier's, lately published,[1] in which the student is led to pay due attention to every possible method of inquiry, and to compare with critical severity their comparative results.

Our handbooks teem with descriptions of the most approved methods of preparation by hardening reagents, but I cannot recall to mind any description of the method to be adopted for examining the brain in sections obtained by freezing, and I therefore purpose describing the method adopted by myself, feeling convinced that it will be fully appreciated by those who have up to the present employed the tedious processes of chrome hardening, and suffered, as I have personally, from the numerous disappointments attendant upon the exclusive use of this method. It appears to have been the opinion of several microscopists who have employed the chrome salts exclusively, that the freezing method was wholly inapplicable to the investigation of the central nervous system, and several authorities in cerebral histology had expressed to me such an opinion. Results obtained by at least one German observer and my own work at the West Riding Asylum prove this view to be incorrect.

The Freezing Microtome.—I was led to the employment of ether for freezing tissues, after having experienced numerous difficulties in the use of the ice and salt mixture. The process was so tedious that a more expeditious method appeared desirable, and I therefore constructed an instrument for freezing with ether spray, and described it in the *Journal of Anatomy and Physiology*, for April 1877. At this time I was informed that an instrument was described by Mr. Hughes in the same Journal, twelve months previously; but finding my microtome simpler in construction, and more expeditious in freezing, I have employed it, with some slight modifications, up to the present time. The woodcut opposite (Fig. 1[2]) represents in vertical section the freezing microtome which I constantly employ, and which I can very strongly recommend for general use. The lower half (*a*) is in principle an ordinary Stirling

[1] 'Leçons sur l'Histologie du Système Nerveux,' par Ranvier.

[2] This instrument can be obtained from Mr. Gardner of Edinburgh.

microtome; the upper half consists of a freezing chamber (*b*) and the section plate (*e*). As regards the microtome, I always insist upon the use of an oval instead of a circular plug (*g*), whilst the screw should be $\frac{3}{4}$ of an inch in diameter, finely worked, and with a milled head at least $\frac{1}{8}$ of an inch wide. The freezing chamber (*b*) should have a false sloping bottom, (*f*) leading to an exit tube (*e*), which conducts off the condensed ether. Extremely simple as this contrivance appears, it is absolutely necessary for success that attention be paid to the following details of construction. The freezing chamber consists of a large hollow cylinder of zinc, slightly over two inches in diameter, and capped with a plate of the same metal. This cylindrical chamber is soldered on to the microtome plug, so as to insure absolute steadiness in working, and it possesses three circular openings, $\frac{3}{4}$ of an inch in diameter, one placed in front of the two others laterally opposite each other. The section plate (*e*) is also made of zinc $\frac{3}{40}$ths of an inch thick, and raised upon the vertical arm (*d*), also made of the same metal. The opening in the section plate should be sufficiently large to allow of the free play of the freezing chamber (*b*) through it without affording any point of contact between the two. With a freezing chamber such as the one described, beautifully large and thin sections may be obtained with ease. The material employed in the construction of the freezing chamber and section plate is a matter of importance, and I met with frequent failures and disappointments when endeavouring to utilise other more workable metals. Brass, above all other metals, was found unsuitable, and zinc alone fulfilled all the requisite conditions. Theoretically it was supposed that the metal chamber should be covered with a non-conducting material, such as felt, wood, &c., and that the conduction between the section plate and body of the microtome should in like manner be cut off; but practically it was found that at the sacrifice of a small amount of ether rapid freezing could be insured and a large number of sections obtained before the tissues became loosened from the freezing chamber. It should be remembered that, to avoid the expense and incumbrance of a special condensing apparatus, we have to provide for the free evaporation and subsequent condensa-

tion of ether in the same chamber, and consequently a sacrifice of about one-fourth the bulk of ether used is sustained. I regard as the requisite of a good freezing microtome the following conditions:—

1. The instrument should be of the greatest possible simplicity.

2. The freezing should be rapid and expeditious.

3. The metallic constituents should be such as to retard thawing of the tissue when once frozen.

4. A minimum of ether should be expended.

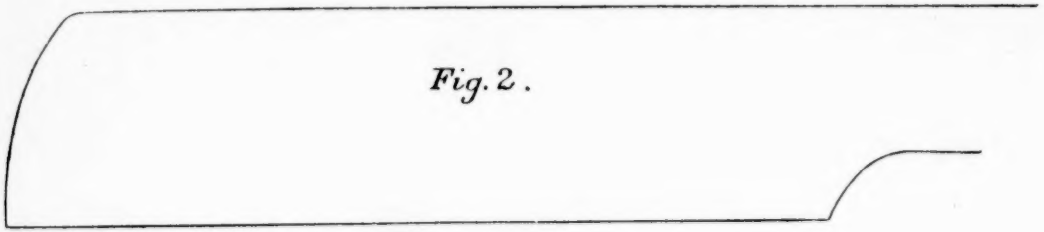
Now I would claim for my instrument a fulfilment of these conditions, as far as is possible, without the employment of an exhausting and condensing apparatus; and I would on these grounds advocate its use by those who require a most satisfactory microtome, and one comparatively inexpensive. The first condition, that of simplicity, is too self-evident to dwell upon; the second also is insured, as the tissue is frozen in less than twenty seconds, whilst it remains adherent to the cover of the freezing chamber for a period sufficing for the cutting of a dozen sections or more. A very small quantity of ether suffices for freezing, and three-fourths of its bulk becomes condensed, and may be collected in a bottle attached to the tube (*e*). I use a graduated bottle for the ether spray, and can thus read off the amount of ether expended in freezing. The costliness of ether has been urged against its employment in ordinary section cutting by one authority of note, and the objection would prove of great weight in case a large amount of ether was lost at each operation; it is but necessary, however, that the instrument be once seen in good working order to dispel all such notions from the mind, as the results obtained are of the first order, and the expenditure of ether very trivial.

Method of Cutting Sections of Brain.—The freezing chamber is lowered until the cap is level with the under-surface of the section plate, and a portion of the tissue, somewhat thicker than the section plate, is laid upon the centre of the metal cap; a couple of drops of water are placed along its edge, which, when frozen, forms a substantial bed, to which the tissue firmly adheres. The nozzle of the spray instrument is now introduced through the opening in the left-hand side of the freezing

chamber, and the spray made to play upon the lower surface of the cap, immediately beneath the tissue to be frozen. A cool draught of air should be allowed, if possible, to play over the surface of the section plate, as it renders freezing still more expeditious. In a few seconds a white band appears at the junction of the tissue and the metal cap, and this rapidly spreads upwards until the tissue becomes blanched and frozen throughout. I invariably freeze up to the level of the section plate, then, with one sweep of the knife, I remove the unfrozen layer above, and so obtain a smooth surface from which to take my sections. A large glass vessel (such as is used for mounting the larger pathological specimens in museums) should be at hand half-filled with water, and the blade of the section knife dipped into it previous to cutting each section. The lower surface of the blade should be passed rapidly over a towel supported on the knee, whilst its upper surface should be allowed to retain a sufficient supply of fluid to float up the section as it is cut. Each section when obtained should be floated off into a vessel containing water. It requires but little skill for the operator to obtain by this method a dozen or more exquisitely fine sections before it is necessary to reapply the spray. The rapidity with which such results are obtained are certainly modified by the temperature of the atmosphere; but with a temperature below 60° Fahr. and in a cool current of air the results will always prove most satisfactory. In extremely cold weather we often find that a little water running off the blade becomes frozen on the section plate, resulting in an uneven surface which wholly prevent section cutting. This obstacle is readily overcome by the use of a little dilute glycerine smeared over the surface of the section plate, care being taken not to extend it to the cap of the freezing chamber.

The Section Knife.—All histologists who have used the hardening methods for obtaining sections of brain and spinal cord will, I think, agree with me when I state that the form of knife used is an important agent in the success of the operation. Thus, when the flat of the blade is placed on a perfectly level surface, the back and edge of the knife should assume *exactly the same plane.* The surface of the blade which is uppermost in section cutting should be sufficiently concave to retain a

Fig. 2.



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fair amount of fluid for floating up the section. Frequent stropping is also indispensable, as the blade should be kept worked up to the finest possible edge. All these conditions must be kept in view when cutting hardened or frozen tissues; but, as the texture of the latter is always such as is most favourable for obtaining the very finest sections, we shall meet with no difficulties in the use of our knife on the freezing microtome—in fact, the discouragements which meet all beginners who attempt section cutting from hardened preparations are wholly absent here, the operation becoming one of the greatest simplicity, wholly independent of any acquired skill. The blade which I almost invariably use measures 5 inches in length by $1\frac{1}{4}$ inch in width. It is made of the best steel, and is concave on both surfaces, but most hollowed out on the surface which is uppermost in section cutting. I add a sketch of a good blade and a section, giving its curved surfaces (Fig. 2, *a*). In the same figure (at *b*) is represented the curved surface of a smaller blade, measuring 4 inches by 1 inch.

Subsequent Treatment of Sections.—We will suppose that we have obtained by the method above described a number of sections cut vertically through the cortex cerebri, and have floated them off into a vessel of water. Each section is now carefully floated on to a glass slide and placed beneath a shade, covering it from dust, the superfluous water being drained off the slide.

a. Osmic Acid Treatment.—A solution of osmic acid, ·1 to ·2 per cent. should be taken up in a pipette, and two or three drops allowed to fall over each section. The osmic acid should be left to exert its action under cover for ten to fifteen minutes, and the sections immediately afterwards floated off in water to rid them of superfluous acid.

b. Staining by Aniline.—The films of brain substance are again received upon the slides and are ready for staining. I employ for this purpose a solution of aniline black—·25 grammes of the granular powder to 100 cb. cm. of distilled water. The solution must be carefully filtered before use. This solution should be poured by means of a pipette on to each slide and over the sections, which become satisfactorily

stained in the course of half an hour to an hour. When it is important to localise each section afterwards, the films should be stained as above on numbered slides, but where such precautions are unnecessary, they may all be stained simultaneously in a porcelain capsule. The aniline solution may now be removed by means of a pipette or syringe, and the films once again floated into a vessel of water; a slight agitation suffices to wash them thoroughly. In this stage, or in the earlier stage, after washing off the superfluous osmic acid, these films form most reliable and excellent preparations for studying the arrangement of the nervous elements in a fresh state, and for the application of reagents in the histo-chemical examinations. For permanent preparations the following process is necessary :—

c. Mounting for Permanent Preparations.—The slides on which the stained sections are placed are now covered by a bell glass, having also introduced beneath it a porcelain capsule containing concentrated sulphuric acid. The sections are thus dried in a fine and perfect film upon the slides in the course of a few hours. A little Canada balsam (dissolved in benzole) is then dropped upon the section and the cover glass adjusted. Such preparations exhibit the structure of the cortex cerebri with a distinctness and beauty unrivalled by any other process which I have seen adopted. Were I to examine critically the various stages of the process now described, and give reasons for the employment of each measure adopted, I should but summarise the many failures and disappointments which I have met in my endeavours to perfect the process. It will, however, prove instructive to note a few facts connected with the history of this process, as they may prove landmarks for the guidance of others. First, then, as to the employment of osmic acid—to what object is it subservient?

Use of Osmic Acid.—If a section of brain be placed beneath a cover glass, separated only by a slight film of water, and examined by a quarter-inch objective, it will soon be apparent that the medullated nerve fibres become greatly altered, as is the case in the peripheral nerves. Myeline exudes around them in its varied and characteristic forms, and eventually the

axis-cylinder becomes perfectly naked and uncovered by the white matter of Schwann throughout its whole extent. If the surface of the cortex *immediately beneath the pia mater* be examined, we observe the same changes taking place in the band of *medullated fibres* which run parallel with the surface of the cortex to the depth of one-third the extent of the first cortical layer. The result is readily conceived. The first layer of the cortex is rendered extremely friable, can only be preserved in fragments, and this with much difficulty; the white matter of the brain is reduced to the same state, and if not carefully agitated and *freed from the abundant exudate of myeline cannot be stained.* Now this simple circumstance afforded me at first much perplexity. What I required was a reagent which, whilst preventing this change in the nerve tubuli, should not interfere with the due staining of the other nervous elements, or alter their delicate relationship. Alcohol and many other reagents were found inadmissible, but in osmic acid I obtained all the requisite conditions. This acid in an extremely dilute solution coagulates the white matter of Schwann, and preserves most perfectly the relationship of the various histological elements of the tissue, nor does it in any way interfere with the subsequent staining by aniline. One word of caution, then, to the histologist. Do not leave your sections of fresh brain in water longer than you can possibly avoid, and counteract its destructive tendencies by the employment of osmic acid.

Aniline Staining—Conditions essential to success.—It will be found that the immediate treatment of fresh sections by aniline black tends to reduce their consistence, that exposure to the action of more concentrated solutions or even the prolonged action of a ·25 per cent. solution will equally result in rendering the film extremely friable and eventually reduce it to a pulpy stringy mass. This is due to the same result, the destructive action in the nerve fibre which proceeds far more rapidly than under the agency of water alone. The greatest difficulty is consequently experienced in washing the preparation from the superfluous reagent, and the increased friability is fatal to all subsequent treatment of these films of nervous tissue. Here again the agent which neutralises the detri-

mental action of the aniline dye is osmic acid, and the consistence of the sections becomes so modified by its employment that staining to any extent by aniline may be adopted, the sections freely washed and manipulated with but little fear of tearing their structure, however delicate in tenuity. In fact, the osmic acid becomes here a reagent of the utmost importance, as we are enabled by its use (*a*) to ward off the destructive action of other fluids on the medullated nerve; (*b*) to give these delicate films the consistence requisite for subsequent staining and manipulation; (*c*) to preserve the normal appearance and relationships of the histological units—conditions which could not be insured by the aid of alcohol and other reagents. We do not in these permanent preparations preserve the dark staining of the medullated tubes characteristic of the osmic acid reaction such as would appear if our sections were mounted in glycerine. Why then do we not employ glycerine in lieu of the process of desiccation? The reason is obvious—glycerine is a powerful solvent of aniline black, nor have I up to the present time succeeded in fixing the dye sufficiently to resist the solvent power of this reagent. But there are limits to the power of aniline black as a staining reagent, and it is essential to give due consideration to this fact. By its agency the nerve cells are rapidly stained, the nuclei taking a deeper tint, whilst the processes are beautifully portrayed, the minutest nerve fibrils faithfully represented, so that the ramifying branches arising from the basal angles of the cells of the cortex are exhibited with extraordinary distinctness. It will be found, however, that connective elements of the neuroglia stain far less readily, assuming a paler tint, whilst the perivascular nuclei are deeply tinted. Then again the medullated nerve fibres before and after entering the cortical layers, as well as that band of double contoured fibres coursing along the outer or peripheral zone of the cortex, remain wholly unaffected by this reagent. Consequently an examination of the white matter of the brain shows us the vessels, their nuclei, and the connective cells as the only elements stained. This, at first sight, may appear strange, since there is no reason for doubting the identity in constitution of the axis cylinders of these fibres and the deli-

cate processes given off from the apex and basal angles of the nerve cells which are so distinctly mapped out by this reagent. A satisfactory explanation, however, can be afforded. If our sections be allowed to remain some hours in water prior to staining, it will be found, on examining the mounted preparations, that the white matter no longer presents the bare appearance above indicated, but is rich in dense bundles of most delicate fibrils running parallel in rectilinear or curved directions towards the cortical layers. Many of these fibrils are broken and curled upon themselves, but the vast majority sufficiently well indicate their nature and their normal relationships.

These fibres are naked axis cylinders beautifully stained by aniline, and afford an instructive study to the histologist. The successful staining is wholly due to the prolonged action of water, whereby the white medulla of Schwann has been removed, which previously formed a powerful obstacle to the admission of the aniline reagent. In recommending this method for the preservation of sections of the cortex cerebri it will be understood, from my former remarks, that I by no means regard such work as the equivalent or substitute of direct examination of the fresh film. Such observations are of incalculable value, and should never be omitted, as they afford information which can often not be obtained from the permanently mounted specimens, and, with a little experience in their manipulation, the nervous, connective and vascular elements may be displayed in all their integrity and beauty.

Relative Merits of Chrome and Fresh Methods.—In advocating the above method for the preparation of the cortex cerebri, I would wish to state a few facts which I believe clearly indicate its superiority to older processes in some very important particulars. In the first place, we must regard it as an expeditious and ready method for the examination of fresh tissues, involving the sacrifice of a minimum of time, whilst securing results of a most reliable character. Should our object be to examine these tissues in a fresh and unprepared state, the films cut from frozen brain afford us the best material both for subsequent testing and for histo-chemical research. If on the other hand we desire to secure permanent specimens, the method

advocated will ensure excellent results. Do any special advantages pertain to this method which we do not obtain by means of preparations hardened in chrome? To this query I may reply very decidedly in the affirmative. Thus it is an indubitable fact that the cortex cerebri is greatly diminished in depth by chrome hardening, frequently being reduced to one-half the depth shown by the method which I adopt. By the latter, in fact, we obtain the natural depth of the cortex, no shrinking occurring if the preparations have been carefully made. Actual measurements, however, must determine this point, and the subjoined table gives the results of comparison of the depth of the cortex in hardened and frozen specimens:—

Average depth of cortex	Man.	Cat.	Calf.	Sheep.
In hardened brain	2–2·5 mm.	1·5 mm.	1·2 mm.	1·55 mm.
In frozen brain	4·836–5·7 „	2·6 „	2·04 „	2·6 „

The depth of cortex in man, the cat, and calf, given above, for hardened preparations, is quoted from Meynert, and the table represents in a marked manner the degree of shrinking undergone in the chrome hardening. This is a matter of great importance, when we consider that, as we descend in the scale of mammalia, we recognise a relative increase in depth of the first layer to the whole depth of the cortex. Meynert supplies us with a list illustrating this important fact, but on contrasting a few of such data with those presented by the frozen specimens we find the point comes out still more forcibly as shown by the following table:—

Hardened by Chrome.	Man.	Cat.	Calf.	Sheep.
Absolute depth of first layer ..	·209 mm.	·348 mm.	·40 mm.	·441 mm
Relative depth to whole cortex	$\frac{1}{8}$–$\frac{1}{10}$ „	$\frac{1}{5}$ „	$\frac{1}{5}$ „	$\frac{1}{5}$–$\frac{1}{4}$ „
Frozen Preparations.				
Absolute depth	·279–·348 mm.	·348 mm.		·441–·558 mm.
Relative depth	$\frac{1}{13}$–$\frac{1}{16}$ „	$\frac{1}{7}$–$\frac{1}{8}$ „		$\frac{1}{8}$–$\frac{1}{5}$ „

A glance at this table reveals the fact that, whilst chromic acid and its salts act powerfully in the direction pointed out, they also affect the cortex of various animals to a very different extent, the degree of shrinking varying with each animal. But apart from the consideration of this corrugating action of chrome reagents, another important fallacy is introduced. In no preparations made by this method have I ever been able to

see the same wealth of structure which is shown by the specimens mounted by the fresh method. Here we find the nerve cells far more crowded and their processes more numerous, more distinct, and more complicated than seen in a corresponding section prepared by the chrome method. Careful examination of this point has led me to the decided opinion that the older method fails to show us anything like the richness of nervous structure which should be seen in our sections, and is certainly antagonistic to the exhibition of the complicated branchings of the various groups of nerve cells. The cells also are stunted, unnaturally angular, and those of the second and third layer present appearances often very different from what is seen in the perfectly fresh section—these appearances, however, being very accurately preserved by the process which I adopt. The disadvantage connected with the process—the want of staining of the medullated fibres running up from the white matter—has been already dwelt upon, as well as a method whereby this difficulty is partially removed. Both old and fresh methods should undoubtedly be employed in examination of the brain cortex, but the ease and rapidity with which results are obtained by the latter will recommend it to histologists for application on a far more extensive scale than would be possible by the older methods.

THE SYMPATHETIC CENTRE.

BY W. HENRY KESTEVEN, M.R.C.S.

Is there a centre from which the vaso-motor or sympathetic nerve force originates in the medulla oblongata or spinal cord? The above question assumes the assertion of the existence, by physiologists, of such a centre. There must be, it is therefore inferred, some grounds on which such assertion takes its stand. Before any attempt be made to answer the question, it will be as well to examine the grounds of the views here implied.

Turning to the writings of those who have asserted its existence, we find that their conclusions are mostly founded on the results obtained by experiments on living animals. One of the most modern writers on the subject, Dr. Julius Althaus, gives expression to these views, in his work on Nervous Diseases, in the following words:—"The chief vaso-motor centre is situated in the medulla oblongata, but there are minor centres for the blood-vessels throughout the entire length of the spinal cord. Faradisation of this organ causes constriction of all arteries below the seat of irritation, while destruction of it produces dilatation of the same, likewise below the seat of lesion. The vaso-motor centres appear to be situated in the anterior cornua of the grey substance of the cord, and the vaso-motor nerves coming from them proceed from the lateral columns through the anterior roots of the spinal nerves to the periphery. That such centres exist all the way down in the cord is shown by the following phenomena: After the arteries have for some time remained distended from section of the cord, they contract again, so as to resume their normal diameter, when the effects of shock, and consequent paralysis of the lower centres, have passed off.

Every further section of the cord lower down causes the same phenomena, viz. at first dilatation, and afterwards contraction, of the arteries. Even after destruction of the lumbar cord, the blood-vessels of the hind parts do not remain permanently or completely paralysed: and we are therefore led to the conclusion that, independently of the vaso-motor centres in the medulla oblongata and the spinal cord, there are local ganglionic centres in the vessels themselves, analogous to the motor ganglia of the heart, which still cause rhythmic contractions of that organ after it has been severed from its centre in the medulla oblongata, as well as from the sympathetic and pneumogastric nerve. After destruction of the higher centres, the lower, and also the peripheral centres, seem to gain in power, through the increased quantity of blood contained in the vessels after such operations."

The above quotation has been selected because it represents the most modern phase which the question has assumed. Subsequent remarks must be considered as directed against the views therein expressed. The first point to be remarked is that the conclusions arrived at are founded on the results of experiments which must at least be regarded as destructive of vital processes. The condition, for scientific purposes, of any animal who has repeated sections made across its spinal cord must, to say the least, be rather anomalous. Taking, however, the conclusions which have been arrived at, it is here meant to be maintained that the results do not warrant the beforementioned deductions, *e.g.* 1st, that there is a vaso-motor centre in the medulla; 2nd, that there are other vaso-motor centres all down the cord, inferred to be in the anterior cornua; 3rd, that there are local ganglionic centres in the vessels themselves, this conclusion being partially founded on analogy; 4th, these peripheral centres seem to gain in power after the destruction of the higher centres, through the increased quantity of blood contained in the vessels after such operations.

The last of these conclusions is somewhat dependent on the foregoing one, in so far as the existence of the ganglionic centres in the vessels is taken for granted.

The results of experiments on which all the above inferences

are grounded, are the constriction of all the vessels below a point faradised, and dilatation of the vessels below a point cut across or otherwise destroyed. If the above results can be shown to be explicable from another point of view the conclusions must at least be weakened.

A study of the connections which exist between the cerebro-spinal and vaso-motor systems seems to reveal another and, as it appears to the writer, a more rational explanation of the above results. It is well known that what is called the sympathetic nervous system consists of a double chain of ganglia arranged on either side of the anterior aspect of the vertebra, these ganglia all giving off nerves which are distributed throughout the system in more or less complicated connection with the nerves of the cerebro-spinal system.

The connections existing between the two systems may well be described as complicated. Without, however, going into any very elaborate descriptions of these connections, it is sufficient for present purposes to divide them broadly into two classes, *central* and *peripheral*.

The *central* are those which unite the ganglia of the sympathetic chain or trunk with the cerebral spinal nerves, called the Rami Communicantes. The *peripheral* are those which exist, as their name implies, between the various nerves passing from either centre. The great point to be observed with regard to these connections is, that at no place does real union take place between the systems without the interposition of nerve corpuscles. The nerve fibres may run together, that is, in the same direction, but they are never actually united unless the nerve corpuscles intervene. Such intervention of corpuscles gives rise to secondary ganglionic formations. These formations vary in accordance with the number of nerve fibres which have to be united. The otic and submaxillary ganglia are instances of small ganglionic formations; but where this coalescence takes place in the neighbourhood of some large viscus, the ganglionic formation is more complex, and has received the name of plexus. Without here entering further into the anatomy of the subject, we pass on to the object which is aimed at and attained by means of their various and complicated connections.

And now the question as to the nature of the force which, taking its origin in the nerve corpuscles, causes the various forms of motion which are necessary for the due carrying on the animal economy, requires to be considered. Dr. Radcliffe, in his work on Vital Motion, proves that "*nerve force*" (so called because those words might mean anything) is simply electricity. It may be said that the change of names has not thrown much light on the subject, but the word *electricity*, although in itself only meaning a force derived from *ἤλεκτρον*, implies at least a force with the characteristics and mode of action of which we are better acquainted than with the indefinite properties of what was called nerve force. Dr. Radcliffe has shown that in a state of rest both nerve and muscle are in a state of charge analogous to that of a charged Leyden jar. *Action is discharge.*

Such is the main result brought out in the work above referred to.

Taking, then, this view of the power which causes the movements requisite to life, we next inquire what is its mode of action. More particularly, How do the sympathetic ganglia fulfil their part in the general economy?

First, with regard to the electro-nervous charge which exists in a state of rest.

This state of charge in the ganglia of the sympathetic trunk is derived from the cerebro-spinal system by means of one of the Rami Communicantes, and to a certain extent is self-existent. It is by these means that the nerve corpuscles of the sympathetic derive their power of action or electro-nervous force. It is, however, one of the functions of all nerve corpuscles to cause modification of the impressions brought to them. The peculiar modification which the nerve corpuscles of the sympathetic cause in the electro-nervous force thus derived from the higher or more specially developed cerebro-spinal system, consists in enabling that force to act upon the involuntary muscular fibres throughout the body. This, then, is the object of the central connection between the systems, the supply of electro-nervous force for the modification rendered necessary for the attainment of the functions of the sympathetic system, viz. contraction of the involuntary muscular fibres.

As, however, it has been shown above, this central connection is not the only one existing between the two systems, there is also a series of peripheral connections. That these latter are as important as the central connections is shown by the fact that, as regards the sympathetic system, they are universal. *That is, there is not throughout the body a single sympathetic nerve fibre which reaches its destination without previous admixture or fusion to a certain extent with nerve fibres from the cerebro-spinal system.* This fusion or admixture is brought about by means of the various ganglia and plexuses which are scattered throughout the body.

The object of such fusion is that the force which is distributed from the central sympathetic ganglia may be controlled. How this control is accomplished cannot be conclusively determined. One explanation which appears feasible is, that at these points of fusion there meet, under certain circumstances, two currents of force. The result of such meeting may be, supposing the forces are equal in all respects, a complete stoppage of both currents, or, if one or the other preponderates, there will be a result corresponding to the preponderance. Whatever may be the *modus operandi*, the result as regards the sympathetic is certain. The peculiarly modified electro-nervous force has not an unlimited or uncontrolled power of action. It is easy to see that were this the case there would be a condition of block throughout the system, a complete arrest of all the natural processes.

Taking, therefore, this explanation of the conjoint action of the cerebro-spinal and sympathetic systems as a stand-point from which to view the results obtained by the experiments mentioned above, it will be seen that they are capable of another explanation.

It is presumed, in the conclusions above quoted, that faradisation causes constriction of the vessels by the stimulation of a supposed vaso-motor centre; and that section causes the reverse, by destruction of, or separation from, the supposed centre. If, however, the state of rest is considered to be, as proved by Dr. Radcliffe, a state of charge, faradisation is likely rather to maintain that state of charge, and, therefore, to prevent action in the parts faradised. As has been shown above, if the action of

the cerebro-spinal nervous current is stayed, that current coming from the sympathetic ganglia in answer to the regular normal stimulation will acquire preponderance. In other words, the involuntary muscular fibres of the blood-vessels will be contracted. In the case of section or destruction in the cerebro-spinal centres, any one who has ever killed a fowl by breaking its neck, and so crushing the medulla, must have been convinced that electro-nervous discharge results, causing violent general convulsions. The cerebro-spinal nervous current in this case acquires preponderance, the result is dilatation. If the destruction be not in a vital point of the cerebro-spinal system, there is nothing to prevent the sympathetic nervous current resuming its share of power when the discharge has passed off. Nervous centres, such as are described in the third conclusion, have never been demonstrated. The existence of such in the heart is not sufficient evidence, unless the necessity for them in the vessels can also be shown. From the above point of view such does not exist, nor does the gain of power in the peripheral centres after destruction of portions of the cerebro-spinal system require to be accounted for by the increase in the amount of blood.

In conclusion, then, it may be said:—

That there is not a direct vaso-motor centre to be found either in the medulla or in the cord.

There is also every reason, on the above grounds, to doubt whether there is any truth in the supposed existence of a centre specially regulating the production of body heat.

Critical Digests and Notices of Books.

Insanity in Ancient and Modern Life, with Chapters on its Prevention. By DANIEL HACK TUKE, M.D. Macmillan, 1878.

WE doubt whether the interest in this work will be as general as it would excite if the accuracy of the statement expressed in the opening paragraph were acceptable to the foot of the letter.

"A remarkable feature of the present day is the interest felt by the public at large in questions relative to insanity. So far from its being confined to the medical profession, legislators, lawyers, clergymen, sociologists, metaphysicians, have their attention forcibly drawn to the subject, and are keenly interested in it."

That there is a widespread public curiosity about insanity is undeniable, but that any considerable section of the public—even the medical profession at large—are keenly interested in it we greatly distrust. Rather it would seem that, with the development of the asylum system, which condemns an ever-increasing number of imperfect minds to the seclusion of their own quarters, the contact of the public, and even of the profession, with the insane is continually diminishing, and rational interest degenerating into morbid curiosity which, irresponsive to systematic information, only relishes the stimulant of some sensational incident. In vain is the obvious platitude reiterated that, since the custom of social life and the law of the land place insane persons under the supervision of medical men, therefore candidates for medical diplomas ought to be examined as to their knowledge of mental diseases. In vain is it urged that a knowledge of lunatics is

of more importance to a doctor than that of plants, and that a study of psychological medicine will be of more value to him, and more useful to the public, than that of systematic botany; that a remote practitioner may never in his life meet with a single dislocation of the hip, but that he will be sure to encounter the unforeseen difficulties of dealing with some scores of madmen. The medical corporations refuse to add the burthen of so needless a study as that of insanity to their curricula, and the means and opportunities of such study for the profession at large have actually diminished; for whereas, in the time of our fathers, insane patients were met with in general hospitals, and one great London hospital at least devoted special wards to such patients, and the teachers and professors of medicine included mental diseases in their courses of lectures, the attendance upon which was compulsory, now all these opportunities for the instruction of the general medical student have been allowed to slide into disuse, and the clinical study of insanity is restricted to the dribbling admission of a few students into asylums by the grace and favour of the authorities, and the teaching to voluntary attendance upon psychological classes, most of which are skeletons, and some of them ghosts.

One great peculiarity of all Dr. Hack Tuke's writing is its precise conscientiousness. There is no author who less needs the disclaimer of dogmatism which we find in his eighty-eighth page. He is the most nice and precise of critics, and he tells us faithfully all that can be said on either side, or on all the sides, if the question is one upon which many facets can be cut. This is mostly a great merit, although it sometimes leaves the impression that he is trying, as the old song says, to "balance a straw." Another consequence of this thoroughness is that he cannot leave anything out, and therefore he commences his present work with a chapter on the "Causes of Insanity in Prehistoric Times." It may be assumed that the men of the drift period had no "books but the running brooks," which, however, are no records, and we are thus reduced to "the question of presumption and probability"—a question which perhaps we might approach by inquiring into the causes of insanity among existing savages. But informa-

tion, even upon this point, which we do not get, might be extremely misleading. The primeval ancestor of Mr. Tennyson, for instance, or of the late Bishop of Oxford—two gentlemen who have expressed great anxiety upon the point—was most likely a very different being either to the greater ape or to a native of New Guinea. Whatever he was, he contained within him the potentiality of becoming "the paragon of animals!" Surely there is no greater mistake than the widespread one that our anthropoid ancestors were like the stereotypes of monkey and of savage man which have come down to us through the æons as they now exist. At least they had within them this stupendous potentiality of development, to which, with capacity and promise of neural perfection, resulting from its very instability of definite form, the penalty of neural deviation into insanity may have been attached. This principle, that the tendency to involution under unfavourable circumstances goes *pari passu* with the tendency to evolution under favourable circumstances, explains many apparent anomalies; the propinquity of genius to madness, for instance, and the mental safety of the inert races of mankind compared with the ravages of mental disease among ourselves.

In his second chapter the author considers the causes of insanity among the Jews and the Egyptians, and in the third and fourth those among the Greeks and the Romans; and the various modes of living practised by these ancient folk are examined with much learning and discrimination, in order to lead to a probable answer to the question whether they were likely to have caused insanity. In the absence of all positive information as to the prevalence of insanity, these chapters come too much to resemble an academical exercitation to be of great scientific value. The Jews in Egypt may have been decimated by madness, or they may have been free from it. Who can tell? And as for guessing, can Dr. Tuke tell us how it was that a nation of slaves, born and bred under the most grinding bondage, acquired none of those qualities of mind which we know that in these days servitude infallibly impresses upon a race? How it was that a warrior race sprang from the flesh-pots and the brick-fields of the Nile? We

know nothing of Jewish madmen, save only of Saul, but we read much of their armies and their victories. How came these about, and what is the effect of slavery upon the mind? In his chapter upon the Greeks and Romans the author leaves this great component of ancient life unconsidered. And yet in Attica there were 400,000 slaves to 130,000 free-born natives and aliens. How small an effect must the classic influences which the author cites have had upon a population thus compounded? The result of the inquiry appears to be, that whereas the causes of insanity were pretty active among the ancients, the elimination of the unfittest was still more so; the duration of lunatic life was brief, and although many imperfect brains may have been begotten, few of them were reared.

Dr. Hack Tuke leaps over the middle ages, with their carnage and conventualism, their plagues and priestcraft, and it is pleasant to pass directly from the tepid zephyrs of classic learning into the crisp breezes of social science. Here there is so much motion from many points of the compass that we can no more than indicate the path of our author in the troubled seas of controversy. On the interesting question whether lunacy is on the increase in England, Dr. Tuke commences with the statement of that exceedingly stubborn fact that whereas twenty years ago there was one lunatic or idiot officially reported to 577 of the population, the latest returns place the ratio as high as one in 370. But he maintains that it remains an open question to what extent this vast disparity is accounted for by the prolongation of insane life and by the increase of population. The latter element might surely be computed with accuracy, and the former with considerable approach to the truth. The average duration of life in asylums is not an unknowable, scarcely an unknown, quantity, and any one possessed of statistical patience and power might surely give us a definite answer to this question, which has been written about in a vague and speculative manner quite long enough. "The apparent increase of insanity is mainly marked among those who become pauper patients;" but this fact "does not prove that there has been anything like a corresponding growth of insanity among the

poor as compared with the rich;" still, "no doubt, mental stagnation is in itself bad, but the insanity arising out of it is more frequently an indirect than a direct result."

The conclusion at which the author arrives, as to the insanity of the higher and lower classes of society, is expressed in the following well-balanced opinion:—

"Alternately swayed by the evidence which presents itself, now in the higher, now in the lower, ranks of society, of the fearful consequences of certain lines of conduct and living, alike ending in insanity, we seem at one time driven to the conclusion that a refined and over-civilised life, and at another that an unlettered one, most favours the spread of mental disorder. The right conclusion is, however, that both present their peculiar perils in different forms, but that the latter, in that form in which it displays itself in England, produces the largest amount."

The final conclusion arrived at on this point is, that, "looking not at the accumulation of lunatics in asylums, but at the admissions, and making every possible allowance for their considerable rise beyond that of the population, it is impossible to deny that there is reason to fear some real increase of occurring insanity."

The relation of insanity to drunkenness is left in the same questionable state. In the chapter on the lower classes drink would seem to be given as an explanation of the pauper predominance, while in the chapter on the higher classes it is recorded that at the Royal Edinburgh Asylum "there is even a higher percentage of insanity from this cause [drink] among the private than the pauper lunatics." It is a question of vast importance, which has been obscured by much exaggeration and misrepresentation, and which needs for its solution other methods and wider views than have yet been taken.

On the influence of intellectual work as a cause of insanity, the author does entertain a strong opinion. He indeed generally uses the term "mental strain," or "overwork," but this is somewhat begging the question. When intellectual work causes insanity, it may very justly be called overwork. But the amount of mental work which ought to be called overwork, and the circumstances in which it will be mischievous, are left in

the greatest uncertainty. That weak brains are injured by work which is only fit for strong ones, and young brains by that which is alone adapted for older ones, may well be believed by any one without the aid of that beautiful essay on the Building of a Brain, by Dr. Robert Clarke, of Boston. It is difficult to find any limit to the work which sound and sane brains may do without injury, but it is also hard to conceive the minimum of work which may safely be encountered by poor, weak, and twisted organisms, with inherited propensities to break down, aided by unwholesome modes of life, and exacerbated by emotional disturbances, by the anxiety and worry, the emulation and envy, of the competitive-examination system. Examine boys, and girls too, by all means, to ascertain that they are not idling time away, nor taking wrong and devious paths of thought; but to urge brains of tender age into racing pace with each other by the whip and spur of keen emulation, is a mode of modern life which is righteously denounced by the author.

"Mental strain in modern life is not only in some cases a cause of insanity, but it very frequently induces general exhaustion, or disease of the heart or some other organ than the brain. If, therefore, lunacy statistics do not supply us with the figures which would enable us to point to the occurrence of insanity as the result of undue intellectual work on so large a scale as might be expected, there can be no doubt the penalty is often paid in other forms."

"Too many hours' daily study and the knowledge of an approaching examination, when the system is developing and requiring an abundance of good air and exercise, easily account for pale and worn looks, frequent headaches, disturbed sleep, night-mare, and nervous fears. When the career of such students does not end in graduating in a lunatic asylum, they lose for years, possibly always, the elasticity and buoyancy of spirits essential to robust mental health."

Yet we doubt that the evil is widespread, or that the youth of England are as a rule overtaught. In our public schools athletics are supreme and learning subordinate, and if those noble peers who recently raised a discussion in the House of Lords on the physical deterioration which scientific teaching

is causing among the officers of our army, had taken the trouble to read the *Field* newspaper, they might well have been surprised to discover the exalted position held by our Engineer officers in the records of the sturdy game of football —the game of all others wherein muscle, wind, and pluck are taxed to the utmost, and in which these puny and enervated students have fought their way through all the football clubs in the kingdom to the very highest places.

The truth seems to be that only the weakest go to the wall, and that with mental overstrain, as with intemperance and other unquestionable causes of insanity, we must look to the circumstances and complications under which the cause acts, for its efficiency. The seed only germinates in prepared ground, "many factors go to make up the sum total of causes of an attack," and "where the constitution has no latent tendency to insanity, you may do almost what you like with it, in this or any other way, without causing this particular disease."

The third part of the work is entitled "Auto-prophylaxis, or Self-Prevention of [from] Insanity." The author does not believe that we are "conscious automata," but that "we can exert our will over our mental processes to a very large extent," and that "in no instance is it more important that men should recognise their own will, and the obligation of employing it to control those really automatic tendencies of the mind which often are strikingly characteristic of insanity." This poor will of ours has much to endure and much to do. Not only, like the domestic cat, does it break all the crockery, but it has to mend it again. "The essence of insanity is a diseased will," writes a recent author, and now we are told that the prevention of insanity lies in the employment of a sane will. For ourselves, we always feel that when the will is brought forward in argument, there is nothing more to be said, and in a double sense we admit the truth of the old proverb, "A wilful man maun ha' his way."

The ninth chapter, on "Warnings of Danger," pleases us the best, being most practical and positive. The warning voice may be listened to, not by the patient alone, who is often deaf, but by those to whom his sanity is as precious as to him-

self; and if the prodromata of mental disease could only be recognised, and the morbid causes and processes resisted, there might be the sure expectation that the annual crop of insanity would be greatly thinned.

The most formidable danger-signal—the drum at the mast-head—is inability to sleep. The foe, shunning the garish eye of day, loves to assail us in the dark, when spectres of the mind "perplex us, worry us, scare us, render us restless and utterly unable to regain our broken sleep." The man whose brain is overwrought—"if he neglect this one symptom—sleeplessness—he is running a great risk of losing control over his mental operations and becoming unable to apply his mind to his duties, the facile descent to actual madness being the sequel."

The author explains the production of morbid wakefulness by "the sympathetic nerves controlling the calibre of the arteries, and the capillaries in the brain lose their grasp upon the vessels, and their powerful antagonist—the grey matter of the brain—persists in keeping up its molecular activity; an activity which may be of a productive description, leading for a time to brilliant results, but more often imperfect and intermittent; and ending at last in vague, irregular, and indefinite mental action."

How it is that the sympathetic nerves come to lose their controlling power on the brain capillaries in the early stages of insanity, is another question to which no answer is suggested, so that the cause of morbid sleeplessness is only put back a stage, if this theory of its immediate production be correct.

Another danger-signal—say, the cone half-mast high—is inability to do the same amount of mental work as before. Yet another is strange unfounded fear about the future; and another indecision of mind not natural to the character.

When these warnings of danger are observed, the author gives the following excellent advice, which if it were followed would no doubt be most efficacious; but which we fear will almost always amount in practice to the advice given to a drowning man to get out of the water. "Let him whose judgment is so bewildered by opposing forces and rival claims, and whose action is paralysed in consequence, promptly and

resolutely determine upon one or other course, either on his own or another's judgment, and exercising all his self-control stick to it without faltering or wavering. If not, he will only plunge deeper and deeper into the mire, which will render him more and more unable to move forward and employ the faculties he possesses to any purpose." The author takes as text for his chapter on the preservation of sanity by means of cheerfulness and mental rest, the wise couplet which, he says, has been taught to us by the school of Salerno :—

> "Si tibi deficiant medici, medici tibi fiant
> Hæc tria; mens hilaris, requies, moderata diæta."

It was, in fact, John of Milan, one of the Salernian doctors who wrote these wise and pretty lines in his Aphorisms, not concerning the prevention of insanity, but the prolongation of life; and here is another of his precepts yet more applicable to the former object:—

> "Si vis incolumem, si vis te reddere sanum,
> Curas tolle graves, irasci crede profanum;
> Parce mero; cœnato parum; non sit tibi vanum,
> Surgere post epulas."

Limits of space forbid us from wandering further in our remarks upon the author's varied pages. Also we cannot quite dismiss the fear that when author or critic writes with prolixity he may be set down as trying to prevent his own insanity by the exercise of that literary composition which the author recommends as a sovereign remedy for certain mental disorders. But madmen themselves can write profusely, and not badly either, as that interesting though imperfect record, 'La Littérature de Fous,' sufficiently testifies, while writings about them are fast becoming a literature all too vast for comprehension, of which, however, this last contribution of Dr. Hack Tuke's, not ponderously scientific, will form a pleasant interlude.

JOHN CHARLES BUCKNILL.

Relazione Statistica Clinica del Frenocomio di S. Margherita di Perugia per gli anni 1874, 1875, 1876. Medico Direttore Roberto Adriani.

The Report of Dr. Robert Adriani, Superintendent of the Asylum at Perugia, gives the statistics of three years, with the results of his experience, and the most remarkable cases during this period. It is evident that Dr. Adriani has the current questions on physiology, pathology, and medicine ever present in his mind, and is a diligent reader of the principal French, German, and English writers who deal with insanity and the diseases of the nervous system. The result is an interesting and valuable report in a quarto pamphlet of 105 pages, one half of which is filled with statistical tables. In framing these tables he has used the classification of Verger which was adopted at the Psychological Congress at Imola. It has the following divisions: Imbecility and Idiotcy, Mania, Monomania, Melancholia and Dementia, Moral Insanity, Circular Insanity, Sensorial or Delusional Insanity (Frenose Sensoria), Hypochondriacal Insanity, Hysterical, Puerperal, Epileptic, Alcoholic and Pellagrous Insanity, General Paralysis.

Dr. Adriani's statistics are very favourable. Out of 286 admissions he had 123 cured, and 16 improved, that is to say, 44 per cent. of cured; subtracting 42 idiots and incurable dements sent from other asylums, the proportion of cures was one half of the admissions. A hereditary tendency was found in 161 patients out of 472. Among the curious and instructive cases described by Dr. Adriani the following seem most worthy of record.

He cites the experiment of Charles Sachs, who isolated a muscle in a frog, leaving it only connected with the nerve which entered it, and then applied electricity both to the nerve and the muscle. In either case there was a reflex general convulsion. He then applied ammonia to the muscle, so far diluted that it would act upon the muscle and not upon the nerve. He again obtained a general reflex movement of the animal, which he concluded to be the result of the pressure exercised upon the nerve fibres distributed through

the muscle. Both Sachs and Remak found a delicate network of nerve fibres encircling or lying upon the sarcolemna, and it is supposed that these are centripetal nerves which communicate to the brain the sensation of accomplished contraction.

Dr. Adriani gives a case which he thinks confirms this view. A man of forty-eight entered the asylum with melancholia and religious delusions. After some days he fell into an insensible condition, the limbs motionless and relaxed; the pulse, respiration, and temperature were normal. It was found impossible to rouse him; irritation of the eye or nostrils produced no effect; and neither cold nor heat applied to the body caused any movement.

Electricity with dry conductors was applied to the most sensitive parts of the skin, and acupuncture and shampooing were also used without effect. It was then determined to test the electro-muscular contractility, and by means of wet conductors the induced current was transmitted to the gemellus internus of the leg. The muscle immediately contracted, and the contraction was followed by a general start of the whole body. The patient recovered as if by enchantment; he saw, heard, moved himself, spoke, and answered questions. He complained of weight in the head, and had a great desire of going to sleep.

Dr. Adriani argues that the man was roused by the sensation of the muscular contraction caused by the application of electricity to the muscles of the leg.

The causes of insanity found in the asylum at Perugia which are not met with in this country, are pellagra and miasmatic fever. The pellagrous form is noted as giving a bad prognosis. All the cases traced to miasmatic infection came from the Agro Romano, the neighbourhood of Lake Thrasimene, and other localities known to be the haunts of malaria. In most instances the insanity seemed to result from secondary oligæmia; but some had suffered from the pernicious comatose form of miasmatic fever, in which the oligæmia was connected with venous congestion and splenic and hepatic hypertrophy, and with a dusty colour of the skin. He conjectures that the continual mechanical irritation of the pigmentary matter in

the walls of the vessels or in the parenchymatous substances of the cortex cerebri had disturbed the nutrition and excitability of the nerve cells, or that, through deposit of the pigment, embolisms had been produced, causing anæmia and partial atrophy of the cerebral substance. In these last cases the insanity took the form of melancholy with hypochondriac delirium, ending in dementia.

Dr. Adriani had a patient, a girl of seventeen, of imperfect intelligence, with predisposition to insanity inherited both from father and mother. Being frightened at the sight of a conflagration, she was seized with melancholy, hypochondriac delirium, refusal of food and persistent stupor. She had to be fed with the stomach pump. During six months of her residence in the asylum she made no progress, when she was seized with typhoid fever of the usual type. During the first seven days of the malady she appeared more lively than she was before, which was attributed to the more rapid irruption of blood into the brain, increasing the activity of the organ; and, with the progress of the fever, her intelligence, instead of becoming weaker, became clearer and more open. When convalescence commenced, the dislike for food completely ceased, the affections revived, and all traces of stupor and delirium disappeared. For a time there still remained some mental abnormality, which took the form of heightened imagination and an exaggerated idea of her own importance; but after some days the cure became complete. Before the subvention of the insanity, her intelligence was weak and imperfect and her affections obtuse, but they now took a new and unaccustomed growth, which has gone on for two years.

Dr. Adriani cites a number of cases where recovery from insanity supervened upon typhoid fever; the most remarkable was an idiot of eighteen years of age, who had never uttered any word save "mamma," but after an attack of typhoid fever, from being of a sad disposition he became gay, and he learned to express his ideas with various words, to sing songs, and to keep in mind the names of things and persons.

In those who died in the asylum of pellagra, the cerebral lesions were various, as in ordinary dementia. The most common were paleness, œdema, and often also atrophy of the

cortical surface, fatty degeneration and greater fragility of the vessels, and serous and bloody effusion under the arachnoid.

In one of his dissections, Dr. Adriani found the corpus callosum deficient. The subject was a man of thirty-eight years of age, a shoemaker by trade. He had been an epileptic for twenty years, and insane for seven years. His grandfather had been demented, and he himself was given to drunken habits. His insanity at first took the form of mania, with hallucinations of hearing and touch, and then subsided into a restless dementia. He died of the consequences of the epilepsy. Before being seized with this disorder, the man could neither be called idiotic nor imbecile, though his intelligence was not good.

Not only the corpus callosum, but also the septum lucidum and the middle and anterior part of the fornix, were found wanting. There was also a general softening of the brain, with œdema and paleness of the cortical substance, effusion of blood under the arachnoid, and inflammation of the middle part of the superior longitudinal sinus.

Dr. Adriani tells us that science possesses twenty-one other instances of want of the corpus callosum, and that four of these were troubled with epilepsy. In many of them, however, other parts of the brain were wanting; where the corpus callosum alone was deficient, as in the cases of Sander and Malinverni, the intelligence was not found to be deficient.

The author has tried arsenic in cases of intermittent insanity, hallucinations, and chronic cerebral congestion. He used it in 89 patients: 20 of these had pellagrous insanity; 7 intermittent mania or melancholia; 9 delusional insanity; 12 general paralysis or congestive insanity (follia congestiva); and 41 various forms of mental alienation connected with anæmia, deficient nutrition, and wasting of the body. Arsenious acid was given, after the formula of Boudin, along with the food, diluted with water and mixed with wine, in a gradually increasing dose of from 2 to 15 milligrammes in the day. Interruptions were now and then made in the administration of the drug. It was generally well tolerated. Of the twenty pellagrous patients thirteen gained great benefit, the others none at all. These last were in a more advanced state of pellagrism. In general those cases derived the most benefit

who were in an anæmic and ill-nourished state. Dr. Adriani believes that no medicine has such power of restoring impoverished nutrition. In many cases, within a week the patients recovered flesh and colour, after iron and other remedies had been found to fail. It ought to be remembered that this was in a southern climate, where diseased conditions are liable to be profoundly modified by paludal malaria. Of the forty-one patients treated with arsenic sixteen were cured. In seven cases of intermittent mania only one derived benefit. The medicine also gave some encouraging results in congestion following upon alcoholic excesses.

WILLIAM W. IRELAND.

School Life in its Influence on Sight and Figure. Two Lectures by R. Liebreich, Consulting Ophthalmic Surgeon to St. Thomas's Hospital. London: J. and A. Churchill, 1878, pp. 36.

IN this admirable pamphlet, which ought to be in the hands of every teacher, architect, and upholsterer, and from which the reading public generally may derive some useful hints, Dr. Liebreich shows that errors in the distribution of light in schools and houses, and in the posture habitually assumed during study, may interfere with the range, acuteness, or endurance of vision, and deform the figure, and so damage health. With reference to myopia, which is increasing in frequency in England as in all civilised countries, he asserts that it is to a great extent caused by the circumstances of school life, and that the percentage of short-sighted children is always greater in schools where unfavourable optical conditions prevail. Amblyopia and astheniopia are also, he thinks, often caused by unsuitable arrangements for work. The observance of a few simple rules which seem hitherto to have been strangely overlooked, and which the Committee of Council on Education in their directions for the planning and fitting-up of schools utterly disregards, will, he believes, obviate the serious evils which he has traced to their source. Light must be admitted from the left hand and from above, and must be sufficiently

strong. Ground and ribbed glass must be used but sparingly and with discretion. Gas jets must be shielded by glass cylinders and reflectors. Every one when reading and writing must sit straight, and have a support to the back. The book or paper must be distant at least 10 inches from the eyes, and must be raised to an angle of 40° for reading and 20° for writing.

A lateral curvature of the spine, dependent upon a twisting of the vertebral column round a vertical axis and accompanied by depression and prominence of the right shoulder, projection of the lower ribs on the right side, with complimentary deviations on the left side and obliquity of the pelvis, which is of exceedingly common occurrence, has its principal origin, according to Dr. Liebreich, in the position in which children are made to sit during school-time, and especially while writing. The anatomical process by which this affection is developed is said to consist in stretching of the ligaments which connect the vertebræ, in a breaking-down of the osseous elements in the middle of the spongy texture of the bodies of those vertebræ upon which the ill-adjusted weight chiefly falls, and in atrophic changes in the over-extended muscles on the convex side of the vertebral column. With much ingenuity Dr. Liebreich differentiates three varieties of curvature corresponding with three different attitudes habitually assumed during writing.

We have no doubt of the correctness of Dr. Liebreich's observations, nor of his conclusions as to the prevalence, physiological explanation, and best methods of preventing the deviation from normal figure, which he has described in so clear and interesting a manner; but we question whether he is justified in ascribing this deviation exclusively to unfavourable conditions of school-life. The special unfavourable conditions denounced may be responsible for many and wide-spread evils, but we hesitate to believe that they will of themselves cause spinal curvature; and that a robust child will become permanently deformed by sitting slightly awry for a few hours daily. When the unfavourable conditions referred to do work mischief, they are probably acting in combination with constitutional delicacy, with vis-

ceral disease, or with other vicious habits of life. That this is so is evidenced by the facts that scoliosis is more frequent in children of the higher than in those of the lower classes of society, in town than in country children, and in girls than in boys. We lately examined upwards of one hundred children, of both sexes, in a rural district of Scotland, attending two schools, in which all the adverse circumstances pointed out by Dr. Liebreich existed in a marked degree. The windows were on both sides, and in one end of the school-rooms the benches were mostly without backs; there was a considerable distance between the seats and desks, so that the children had to sit on the edge of the former and lean forward when writing; and the desks were disproportionately high. In not one of these children could we detect scoliosis. The figures of all of them were not, of course, strictly symmetrical; but no deviation from the normal standard which could be dignified by the name of a deformity was discovered. They were straight, sturdy, active children. But they came of a hardy stock, lived on a frugal but wholesome dietary, in which oatmeal bulked largely, spent much time in the open air and in out-door work and games, and walked on an average three miles a day to and from school. It is in the absence of such healthful modes of living out of school, or in the presence of degeneration, that scoliosis shows itself, and that the precautionary measures indicated in the pamphlet before us become necessary. We do not mean to extenuate negligence as to the maintenance of a proper posture during educational drill, nor to justify a disregard of anatomy in the manufacture of school-furniture; but what we wish to insist on is, that errors of bearing in school hours are almost innocuous unless they co-operate with other morbific or deforming agencies, and that these latter may be to a great extent compensated by plenty of freedom, exercise, and play out of school hours. More important than the shape of the desk and bench is the quantity of desk and bench to which young children are subjected. When that is not excessive, nature will do much to avert scoliosis, and to preserve a useful symmetry of figure. The feelings of a child that is not diseased or grievously over-taxed in strength may be trusted to do much for bodily equilibrium, and to make up for prolonged strain

by prolonged relaxation. Sleep, too, will do something to readjust the balance; for it has always seemed to us that the decubitus at night has a reference to the prevailing posture by day, and that sleep not only

"Knits up the ravell'd sleave of care,"

but undoes muscular knots and smoothes out osseous twists. But in order that it may accomplish this, sleep must have fair play, and the disposition of the body and limbs must be determined by the sense of fatigue, and not by mechanical, thermal, or other considerations. Dr. Liebreich would be doing a public service if he would extend his criticisms on school-furniture to school-dormitories, and would impress on those who are responsible for their arrangements the importance of providing commodious, comfortable beds, and a sufficiency of bed-clothes, and of regulating their temperature with a regard to seasonal changes and the physical condition of their occupants, so that children may not have to huddle themselves together, to roll themselves up like hedgehogs, or to put their heads under the bolster in the pursuit of warmth.

Dr. Liebreich concludes his second lecture by a sketch of the ugly disfigurement produced by a fashionable stay-maker, who has a great reputation for curing scoliosis. Could her aristocratic victims but see themselves as the anatomist sees them, they would never "court an amorous looking-glass" again.

J. Crichton-Browne.

Dr. Féré on Cerebral Topography. Note sur quelques points de la Topographie du Cerveau. Mémoires de la Société de Biologie de Paris. 2me Série, t. iii. Fascic. 3, p. 91.

It was Gratiolet who, in 1857, was first to conceive the idea of discovering the relations of the sulci of the brain to the overlying sutures of the cranium. He made moulds in plaster of Paris of the cranial cavity, but the depressions and projections of the internal surface of the skull do not suffice to give an exact idea of the form and relations of the cerebral convo-

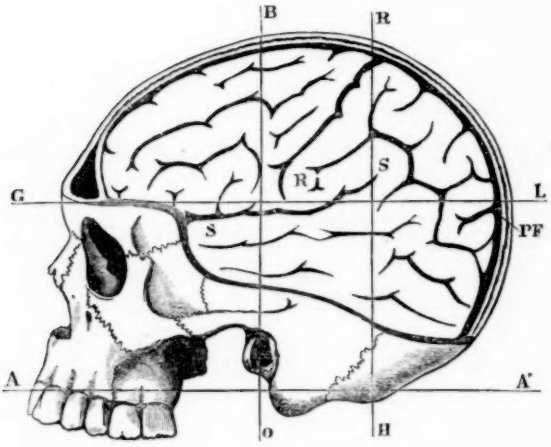
lutions, and he was accordingly led to believe that the fissure of Rolando was parallel and subjacent to the coronal suture. In 1861 M. Broca embarked upon fresh researches with a totally different method, viz., by drilling holes at the exact position of the points of intersection of the various sutures of the cranial vault—at the *bregma*, the apex of the *lambda*, and at the external extremity of the coronal suture. Through these holes he introduced pegs into the subjacent cerebral substance, and was thus enabled to witness the fact, by the examination of eleven subjects, that the fissure of Rolando lies much posterior to the suture in question. In 1874, M. Heftler presented to the Medico-chirurgical Academy of St. Petersburg a thesis on this subject. His procedure, which was carried out on ten subjects, though quite novel, was somewhat complicated, consisting as it did of taking casts of the scalp and cranium successively, and afterwards projecting a sketch of the convolutions upon a drawing of the casts. M. Heftler, though ignorant of M. Broca's researches, came to conclusions in accordance with those arrived at by the French professor; but it is to be regretted that the former has not given any guide whereby we may recognise upon the living body the salient points which he has been led to define. M. Féré, a pupil of Prof. Broca, particularly with a view to the localisation of surgical lesions of the brain, has made a number of observations upon persons of both sexes, viz., upon fifty-four females, mostly above sixty years of age, at Salpétrière, and at Bicêtre, upon forty men of about the same age.

The fissure of Rolando, as M. Broca has already pointed out, is far from approaching so near to the anterior extremity of the brain as is generally received. If a brain be examined after removal from the skull, the convexity of the hemispheres will be seen to tend to become plane again, especially at their posterior part, though this region has a very pronounced curvature, when the brain is lying *in situ*, in the cranial cavity; and the fissure of Rolando would accordingly seem to be advanced to a proportionate degree as the brain is more prolonged and flattened out. In order then to determine its exact position the brain must be measured *in situ*, the head being kept raised as much as possible. It is better, too, to

leave the *dura mater* intact, only incising it just sufficiently to lay bare the fissure, so as to preserve, as much as possible, the shape of the brain; but such results cannot be properly attained, until after the head has been frozen. In old subjects the sutures are often obliterated, but they can be made visible again by moistening the denuded bone with some colouring matter, such as ink, which readily penetrates into any interstices. The external perpendicular fissure has pretty constant relations with the summit of the occipital angle, for thirty-nine times out of sixty-two it corresponded accurately with it. Further down is the occipital lobe, which descends as far as the external occipital tuberosity, while in front are the quadrilateral lobule and the supero-parietal. The perpendicular fissure lies on the same horizontal plane as the anterior extremity of the brain. The fissure of Rolando is far from being parallel and subjacent to the coronal suture, as Gratiolet thought. In the female its posterior extremity is, on an average, forty-five millimètres behind the *bregma;* this distance being a little longer in men, measuring about forty-seven or forty-eight millimètres, according to the observations of M. Broca and the author of this paper. This fissure, though much more oblique than the coronal suture, tends to approach the latter at its lower extremity, but terminates at least one centimètre higher than the suture, as it never descends as far as the fissure of Sylvius.[1] The external extremity of the coronal suture corresponds to the interval of the two folds of the third frontal convolution. From the situation of the fissure of Rolando it can be deduced that the anterior ascending parietal convolution [ascending frontal] and the posterior part of the frontal convolution lie behind the coronal suture. All the parts, then, lying between the fissure of Rolando and the perpendicular fissure are covered by the parietal bone. All these parts, or nearly all the parietal region of the cerebral surface, derive their blood supply from the Sylvian artery and

[1] M. Féré's statement should be qualified. Prof. Turner says ('Convolutions of the Human Cerebrum Topographically Considered,' p. 10, Edin. 1866), that he has "more than once seen the fissure of Rolando prolonged so far downwards as to be continuous with that of Sylvius." Prof. Macalister, too, records a similar case (*Proc. Dublin Univ. Biol. Ass.*, vol. i. No. 2, p. 25).

its branches, according to the observations of M. Duret. In order to determine the posterior extremity of the fissure of Rolando R., which lies at an almost constant distance behind the *bregma*, it suffices to fix the situation of the latter. This lies on the vertex of the skull where this is cut by the upper end of a normal, O B, drawn perpendicularly to the line called by craniologists "alvéolo-condylien" A A', and passing through the middle of the external auditory meatus—the "auriculo-bregmatic" line of Busk.[1] If a line R H be drawn parallel



to the latter from the upper (or posterior) extremity of the fissure of Rolando this will almost exactly cut the *asterion*, or point of junction of the occipital, temporal, and parietal lobes.[2] This line will also indicate the posterior limit of the Island of Reil. A little behind lies the posterior [superior] extremity of the fissure of Sylvius S S and the "Pli courbe." The perpendicular fissure P F answers in most cases to the *lambda;* and here there is, in the majority of skulls, a characteristic angular

[1] According, however, to Dr. Topinard, in his 'L'Anthropologie,' p. 252, Paris, 1876, the *bregma* is the spot where the coronal suture reaches the middle line of the cranium.

[2] The *asterion* is thus defined by Dr. Topinard, *Ibid.*: "point de rencontre, en arrière l'apophyse mastoïde, des trois os pariétal, occipital et temporal."

projection. This fissure may also be indicated by the posterior extremity of a line G L passing from the *glabella* to the occiput, and running parallel to the "alvéolo-condylien" datum line.[1]

M. Féré makes no allusion whatever to the valuable researches made by Professor Turner in the same direction, the results of which were published in the *Journal of Anatomy and Physiology*, Nov. 1873 and May 1874 (vols. xiii. and xiv.), and have been later fully referred to at the end of the last chapter of Professor Ferrier's 'Functions of the Brain,' Lond. 1876.

JOHN C. GALTON.

Nervous Diseases, their Description and Treatment. By ALLAN M'LANE HAMILTON, M.D. London: J. & C. Churchill, 1878, pp. 504.

DR. M'LANE HAMILTON may be congratulated on having produced a concise and practical book on nervous diseases, and on having thus achieved the object which he prescribed to himself on commencing the laborious literary task, the fruits of which now lie before us in a large, well-arranged and well-printed volume. This is unquestionably the best and most complete text-book of nervous diseases that has yet appeared, and were international jealousy in scientific affairs at all possible, we might be excused for a feeling of chagrin that it should be of American parentage. A glance at its pages shows that the greater bulk of the material of which it is composed has been derived from English sources, and it might naturally have been anticipated that the digestion and compilation of that material would be accomplished in England. This work, however, has been performed in New York, and has been so well performed that no room is left for anything but commendation. With great skill Dr. Hamilton has presented to his readers a succinct and lucid survey of all that is known of the pathology of the nervous system, viewed in the light of the most recent researches. In so compre-

[1] The woodcut is copied from Fig. 2 of the plate which accompanies M. Féré's paper.

hensive an undertaking, involving such a number of moot questions, it is of course inevitable that criticism should be challenged on many pages. Frequently we find ourselves differing from Dr. Hamilton's conclusions and recommendations; but to discuss the points of divergence would be to enter upon details, and to leave untouched the general scope of the volume. From the preliminary description of the methods of examination and study, and of the instruments of precision employed in the investigation of nervous diseases, up till the final collection of formulæ, the book is eminently practical. It brings together much scattered information in a methodical and well-balanced manner. We think, however, that it would enhance its usefulness if, in future editions, Dr. Hamilton would give more prominence than he has now done to the psychical symptoms of nervous diseases; if he would supply specific indications for treatment, and fuller directions for the employment of his formulæ, in the manner of Tanner's 'Practice of Medicine'; and if he would amend his pictorial illustrations, which as they now stand are the weakest part of his work, having neither artistic nor diagrammatic merit.

J. Crichton-Browne.

Clinical Cases.

THE BRAIN IN CONGENITAL ABSENCE OF ONE HAND.[1]

BY W. R. GOWERS, M.D.,

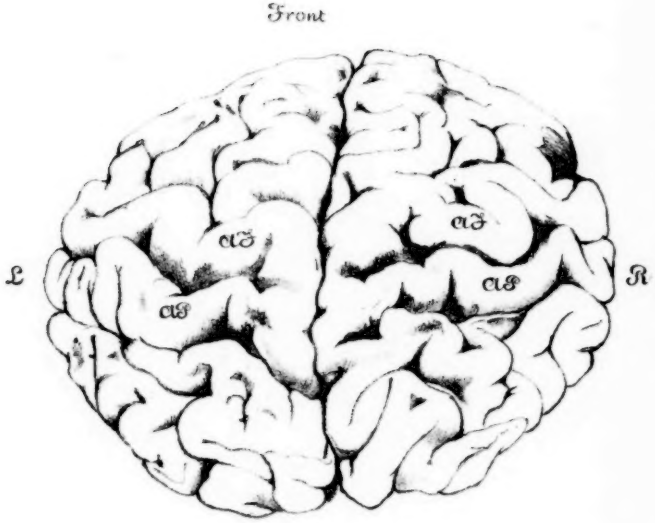
Assistant Professor of Clinical Medicine in University College.

THE correlation of experimental and pathological facts of the localisation of function in the brain is at present far from complete. The following case is interesting as presenting a very close correspondence between the cerebral locality affected in a case of congenital defect of the hand, and the region of the brain to which, according to experiment, movements of the hand seem to be specially related.

The brain, a photograph of which is represented in the accompanying figure, is that of a man, aged forty, whose left hand was absent from birth. He died in University College Hospital from general paralysis.

A careful dissection of the arm was made by Mr. V. Horsley, who found that the forearm bones were normally developed and that at their extremity there was only an irregular mass of bone apparently consisting of the two rows of carpal bones very imperfectly developed and anchylosed together except at one point. The radial third of this mass was capable of a slight rotation around the long axis of the remaining two thirds. Both pieces were connected by strong ligaments and the surface was irregularly nodulated. Over this surface was a fibrous capsule into which both the muscles of the forearm were inserted, and which was movable upon the bones. All the muscles of the wrist and hand were present except the extensor minimi digiti. All were inserted into the capsule over the carpus with the exception of the following. The

[1] Read before the Royal Medical and Chirurgical Society of London.



Frontal and Parietal lobes of a brain in a case of congenital absence of the left hand: viewed from behind. A.F. A.F. ascending Frontal convolutions. A.P. A.P. ascending Parietal convolutions. The two latter are seen to present a marked disparity in size in their middle thirds, outside the letters.

From a Photograph.

flexor carpi radialis was inserted partly by a slip to the capsule and partly by another into a nodule on the radial side of the anterior surface of the malformed "carpus." The flexor carpi ulnaris was inserted into a similar nodule on the ulnar side. The extensor indicis was inserted partly by a slip into the radial part of the capsule at its posterior border, and by another slip into the ulnar side of the posterior "carpal" surface.

The nerves and veins of the forearm were normal. The arteries were normal, with the exception that they did not anastomose at the wrist.

The two hemispheres of the brain were nearly of the same size. On comparing the individual lobes and convolutions of the two sides, with one exception, little difference was apparent. The frontal convolutions were nearly of the same area: on the right side they were individually somewhat smaller, but more complex in arrangement, and from a careful comparison of the two sides it seemed probable that there was little difference. The ascending frontal convolutions were quite equal. Between the two ascending parietal convolutions a marked difference existed. At their origin at the longitudinal fissure, for the first inch of their extent, they were nearly equal in size,[1] and continued nearly equal for the upper inch and a half. In the next (middle) two inches there was a very marked difference, the right being a narrow single convolution and the left broad and depressed by a slight secondary sulcus. The measurements of a transverse section of the two convolutions just outside the spot at which the letters are placed were—the right ·35 inch, and the left ·65 inch. The lowest extremities of the two convolutions were equal in size. No disparity in size between the convolutions of the rest of the parietal or occipital lobes could be detected.

A careful examination of the central ganglia of the brain showed no disparity in size.

A microscopical examination of the two ascending parietal convolutions showed very little difference in their structure. The grey layer was of equal depth and presented layers of nerve cells of the same thickness, and in which the cells of each kind, small and large, were equally numerous and conspicuous on each side.

Remarks.—The chief interest of the case arises from the fact that the diminution of size of the ascending parietal convolu-

[1] In the figure the upper extremity of the left convolution appears larger than the right, but this is in consequence of their connection with the convolutions of the superior parietal lobules.

tion on the opposite side of the brain occupies precisely the area, stimulation of which, according to the experiments of Ferrier, upon monkeys, causes movements of the opposite hand. I am not aware that the brain has been examined in any similar case. In several instances, in cases of old amputations of the arm, an atrophy has been found, but it has been slight, and has not been uniformly localised. The strict limitation of the atrophy to the hand-region in this case affords striking support to the experimental results. The fact that the structure of the two convolutions did not differ, may be associated with the circumstance that the forearm muscles for the hand were present, although they could not have been used; or it may be due to the fact that the representation of parts in the brain is nowhere exclusive, but only preponderant.

TWO CASES OF MICROCEPHALIC IDIOTCY IN ONE FAMILY — CONVULSIONS OF MOTHER DURING PREGNANCY.

BY HERBERT R. O. SANKEY, M.B. LOND.,

Assistant Medical Officer to the Warwick County Lunatic Asylum, late Resident Medical Officer to the National Hospital for the Paralysed and Epileptic.

By the kindness of Dr. Parsey, I am permitted to describe the two following cases of microcephalic idiotcy at present under his care. I am aware that, without post-mortem examination, a description of these cases must of necessity be very imperfect. Since, however, many years may not improbably elapse before such an examination could be made, I venture to publish the following clinical record, in the hope that it may be found to contain some points of interest.

Anne G., æt. 19 years, and Emily G., æt. 11 years, two sisters, were admitted into the Warwick County Lunatic Asylum in the years 1870 and 1872 respectively.

The family history, the greater part of which was obtained from their mother, is as follows:—

The patients' paternal grandparents were first cousins. Their father, who is a shoemaker by trade, also married his cousin. He is described as a sickly-looking man, of a nervous temperament, and is said not to be possessed of the average degree of intelligence. The mother also is a woman of no great mental activity. Neither parent has any insane relations; the youngest child, however, is, in all probability, like the two patients, a microcephalic idiot, but no other members of the family are idiotic; there is, however, a strongly marked family tendency to hysteria and nervous irritability generally. The mother suffered from fits during certain of her pregnancies, and, as will be seen from the following account, her convulsions bear an interesting relation to the idiotcy of her children.

During her first pregnancy Mrs. G. had one slight fit.

The child, however, though strumous, has not grown up an idiot. While pregnant with her second child, the elder of the two patients, their mother suffered from severe epileptiform convulsions. The third pregnancy gave rise to a healthy child, and was not accompanied by fits, but during the gestation of Emily, the younger of the two patients, and the fourth child of the family, Mrs. G. again had fits. Two miscarriages then succeeded, and then four children were born, who, though weakly, were not idiotic, no convulsions occurring during these pregnancies. During the gestation of the youngest child, however, now only two years old, the mother again suffered from epileptiform seizures. Though this child has not been examined, there is good reason to suppose that it is an idiot, since its mother says that it has a small head, and that it does not take notice, nor attempt to speak. Like others of the family it is physically a weakly child, and suffers from enlarged glands about the neck, &c. Nevertheless the mother seems positive that this child is more than ordinarily backward in the development of its mental faculties.

The parents are temperate people, they live in a small country town, and have always had sufficient food and clothing.

The father's brother died of phthisis, but no other members of the family are known to have suffered from that disease.

The elder sister, Anne G., now 19 years of age, has always enjoyed good bodily health, though she has been an idiot from birth. She has never suffered from fits, nor had any serious illness. When admitted into the asylum she was 11 years of age. Her stature was at that time not below the average. Her limbs were well proportioned. Her head was remarkably small, but its dimensions were not recorded. All the cranial sutures were closed. Her mental condition appears at that time to have been very similar to its present state. Her general health was good. She ate well, but was not able to feed herself. She was dirty in her habits, and passed her dejections without regard to time and place. Though spiteful at times, her temper was then pretty good. She is described as lively, and was pleased at being noticed.

With advancing years she became more irritable and morose in her temper. During the three years subsequent to her admission she grew a little, but since the expiration of that time little or no growth has taken place. In 1873 her head measured 14 in. in circumference.

During the next three years there seems to have been some slight advance in her mental condition. She learned to use articulate sounds, but not to speak intelligibly. She became however more spiteful and passionate. In her bodily con-

dition there was no great change, except that in 1876 the catamenia became established.

At the present time her height is 3 ft. 10½ in.; from her habit of stooping, however, she looks much shorter. She is rather stout, and weighs 63 lbs. She is still dirty in her habits, and allows her saliva to run from her mouth. Her most striking feature is her small head. Her forehead slants backward, so much as to be nearly in a straight line with her very prominent nose. Her incisor teeth project considerably, her chin is small, and the cheek bones are high and wide—a combination of features which makes her expression resemble that of a beaver, or water rat. Her occiput is so small that it scarcely at all overhangs her neck posteriorly. The cranial sutures are all closed, and there are no inequalities to indicate their position, so that the share taken by each bone in the formation of the skull cannot be determined.

The occipital protuberance is not well marked. The occipital bone appears at first sight as if it were tilted forward, so that the protuberance is raised to a point higher than normal. Below the most prominent part (Fig. I.) there is a depression about an inch in length, and half an inch wide, and a quarter in depth. But at this point the bone feels subcutaneous, and no ridge can be felt, nor can any ligamentous band be distinguished, even on flexing the head to the utmost, extending to the protuberance from the spines of the vertebræ. The following are the chief measurements of the head:—

Greatest circumference	14½ in.
Greatest antero-posterior diameter	4⅜ „
Distance from one auditory meatus over the vertex to the other	9 „
Longest transverse diameter	3⅝ „
Distance from root of nose to most prominent point at occiput	7⅜ „
Transverse diameter at mastoid processes	3 11/16 „
Distance from auditory meatus to middle of brow	3⅜ „
Do. do. to prominent point at occiput	3½ „
Transverse diameter from zygoma to zygoma, from a point just anterior to articulation of jaw	4⅛ „
Distance from chin to vertex	6½ „

Her gait is peculiar; at first sight it seems to resemble that of paralysis agitans, but on more careful examination it is seen to resemble it only in the fact that the body is bent forward, and that the patient frequently runs. The feet are stamped rather heavily upon the ground, and her legs are moved in a very slovenly manner, but there is no real festination, nor are the knees bent to an unusual degree. She carries her thorax much more forward than is natural, and the head is thrust forward, though the neck is extended so that a straw hat placed on the top of the head when she walks has its brim

resting flat on her back. In fact, the position of her head is very like that of a quadruped.

Her movements are clumsy and, as a rule, slow. She takes a considerable time before, as it were, she decides upon performing any voluntary movement with her hands; and though she is quite capable of making rapid movements, she usually moves but slowly: for example, when the callipers were applied to her head for the purpose of measuring it she invariably raised her hand to remove the instrument, but so slowly did she do this that there was generally ample time to adjust it to her head before she took hold of it. She will, however, clap her hands and shake them, when pleased, quite rapidly. In clapping her hands, however, I have noticed that she, unlike a healthy person, holds her two hands in different, and often in awkward, positions, so that the two palms do not meet in such a way as to produce a sound.

The patient spends most of her time in running about the wards and garden, rarely sitting down except at meals.

Her temper is now better than it was two or three years ago, but she is still passionate, and, when thwarted, will kick and bite most violently. She cannot speak at all. The few articulate sounds she utters are meaningless. She, however, understands much that is said to her—much more, indeed, than would at first be supposed.

Some idea of her degree of intelligence can be gathered from the following incident: She was eating some sweets when the nurse said, "Go and fetch Maria (an epileptic idiot), and give her some too." She at once ran away—went out of the door of the ward, which she opened herself, and ran round the garden, in which there were at least twenty or thirty patients for the most part dressed alike. She went into all the corners and looked everywhere where she thought Maria might be; not finding her in the garden, however, she came back to the ward, still searching, going first into an adjoining room. When she at last discovered her sitting on the floor, she pulled her up and pushed her to the table, and put a sweet in her mouth; and she showed some slight but unequivocal signs of mortification when Maria, after all her trouble, would not eat it. Though she knows Maria and several other patients by name, she does not seem to know her sister when alluded to as "Emily"; nor does she know the nurse who for some years has constantly attended to her, when alluded to by name or spoken of as "Nurse." That she does recognise her is evident from the fact that she obeys her better than anyone else.

She also knows some few objects by their names. She is able to rub and dust the furniture; and if told to dust a chair, she will not, as a rule, operate on a table, or *vice versâ*, though

she sometimes makes a mistake. When bidden, she will go to the cupboard and get her bonnet, which she knows from all the others, though they are made of the same material. If her bonnet is not in its place, she will carefully examine all those in the cupboard over and over again, and finally wander about the room looking for it.

Although she never makes any attempt to speak, she will, when given anything she likes, make a series of small courtesies, and will, when told to do so, clap her hands, which, as before stated, she does in a very clumsy manner.

She possesses but little power of attention, and if shown anything she is as likely as not to go away before the exhibition is complete.

She does not exhibit to the slightest degree any tendency to imitate the actions or behaviour of others.

She is very fond of carrying a piece of string or cloth about in her hand, but, strange to say, she seems to care no more about coloured objects than about others. I never have been able to notice that she liked a piece of red or yellow cloth better than a black or white piece, nor have I been able to obtain any evidence that she had any idea of numbers.

Her memory does not seem very active; in fact it seems much less perfect in her than in her sister. Though I measured her head one day (a process she disliked), she did not keep out of my way, as her sister did, when I returned the following day with the instrument; nor did she seem to remember that I had given her sweets. She seems to like music, but is not excessively fond of it.

I have ascertained that she is capable of experiencing, to some extent at least, the following emotions:—

Fear. She is very timid, and especially so of fire, and runs away from and screams at a lighted taper, though this is daily used to light the gas.

Affection she exhibits, to a small degree, towards several patients who take notice of her. She evidently likes to be with them in preference to others.

Rage she feels to a marked degree, biting and kicking the object of her anger.

She is revengeful, and will return to attack the offenders if prevented in her first attempt. After being angry, she is dull and sulky for some time, and mopes about, hanging her head. If scolded by the nurse, she shows signs of sorrow, and retires to stand in the corner.

She is possessed of a considerable degree of curiosity, and will inspect with interest anything new, and will direct the attention of others to it. As an instance of this, I may mention that on one occasion the patients were to have their

tea in the ward, the dining-hall being unavailable; both she and her sister ran to some one who entered the ward, and who had not seen the preparations, and pointed out the teacups.

Pleasure she shows by smiling and by laughter, and by running about with her arms flexed at the elbows, shaking her hands, and by uttering, from time to time, articulate but meaningless sounds.

She seems quite incapable of feeling sympathy, nor is she known ever to have exhibited jealousy. She will show her mortification if she is not given sweets while others are, but she never shows any illwill to those more lucky than herself. So far as is known, she has never exhibited any sexual propensities whatever.

As may be supposed from the above account, attempts to teach her have not met with much success. She has learnt to feed herself with a spoon, though all attempts to make her clean in her habits have failed utterly. She allows the saliva to run from her mouth.

She sleeps well. When asleep she lies on her side, curled up with the knees applied to the abdomen and the hands upon the chest; in fact, she assumes the fœtal posture.

Her organs of the special senses are all in a perfectly healthy state.

Her heart and lungs are also quite healthy; her extremities are well supplied with blood. Digestion is satisfactorily performed, and the excretory organs are healthy.

Her muscles are well developed; she is pretty strong; her limbs are thick and short.

Her sexual organs are matured; she menstruates with great regularity, and her mammæ are largely developed.

Emily G., æt. eleven years, was admitted when only four years old. Previously to admission she had had convulsions. Her general health had not been nearly so good as that of her sister. When admitted she was small for her age, and had a head very much flattened posteriorly, and of relatively smaller size than her sister's. All the cranial sutures were closed. She was unable to talk; nor could she feed herself. She allowed the saliva to run from her mouth, and passed her dejections under her.

She was nevertheless lively, and liked to be noticed, but was timid and afraid of strangers, like her sister. She was also passionate.

Her general health was not good, and she was subject to bronchial catarrh. During the first year of her life at this asylum she had several attacks of diarrhœa, and several times was under treatment for chest complaints.

She however learnt to perform various acts, and could feed herself with a spoon, and became less dirty in her habits, and learned to understand much that was said to her.

Her health improved as she grew older. In 1873, that is, when she was five and a half years old, her head measured 13¾ in. in circumference. Since that time no great alteration has taken place in her habits or character.

Emily G. is now eleven years old. She is 3 ft. 6 inches in height, and weighs 44 lbs.

She bears a striking resemblance to her sister Anne.

Her head, like that of her sister, is very small.

The depression at the posterior part, described as being present in the head of Anne, is in her head much more marked (Fig. II. P.) Her palate is, like that of her sister, much arched, but not to so great a degree. Her teeth are in good order; she is about a year behindhand in her dentition. Her nose is prominent and her chin recedes, but the face is much narrower, and her expression is less like that of a beaver than that of a mouse. Her eyes, unlike those of her sister, which are small and deep set, are prominent.

Her usual attitude is even more stooping than that of Anne.

The measurements of her head are as follows:—

Greatest circumference	14½ in.
Greatest antero-posterior diameter	4¾ „
Distance from auditory meatus over vertex to other meatus	8½ „
Longest transverse diameter	3⅝ „
Distance from root of nerve to most prominent point of occiput	7¼ „
Transverse diameter at mastoid processes	3½ „
Distance from auditory meatus to middle of brow	3⅜ „
Do. do. to prominent point of occiput	3¾ „
Transverse diameter from zygoma to zygoma, from a point just anterior to articulation of the jaw	3¾ „
Distance from chin to vertex	6¼ „

Her gait resembles that of her sister, but is altered to some extent by the presence of talipes varus of the right foot, for which she wears an instrument. Her movements are much more active and childlike.

Her general behaviour does not, however, greatly differ from that of her sister, but she is more often alone, and is fond of nursing a doll, which Anne will not do.

She is able to say a few simple words. For example, she says "tata" when going to bed. She knows even more people by name than her sister, but it has not been possible to teach her to dust the furniture. She, however, knows her own clothes, and will put some of them on.

Her power of attention and her memory are much better than her sister's.

She has no greater liking for bright colours than has her sister.

As an instance of her dislike for strangers, and as an example of her power of memory, the following incident may be related. She had been taken to the store room to be weighed. She saw there an idiot, whom she sometimes, though not often, saw at a distance in the dining hall. With him, when asked, she shook hands, but she screamed at the sight of the store keeper whom she never saw.

Like her sister, she has not the slightest idea of numbers, nor does she care about music; in fact she cares less about it than Anne does.

Affection she does not exhibit to so great a degree as her sister; nor is she so easily made angry, nor so violent when angry. She does not appear to be revengeful

When scolded she is evidently sorry.

She smiles and laughs when pleased.

I have never noticed any evidence that she is capable of feeling compassion or sympathy.

Her mode of sleeping is exactly like that of her sister.

She is evidently scrofulous, and has had at various times enlargements of glands, &c.

Her heart and lungs are normal, however, and her digestion fairly good.

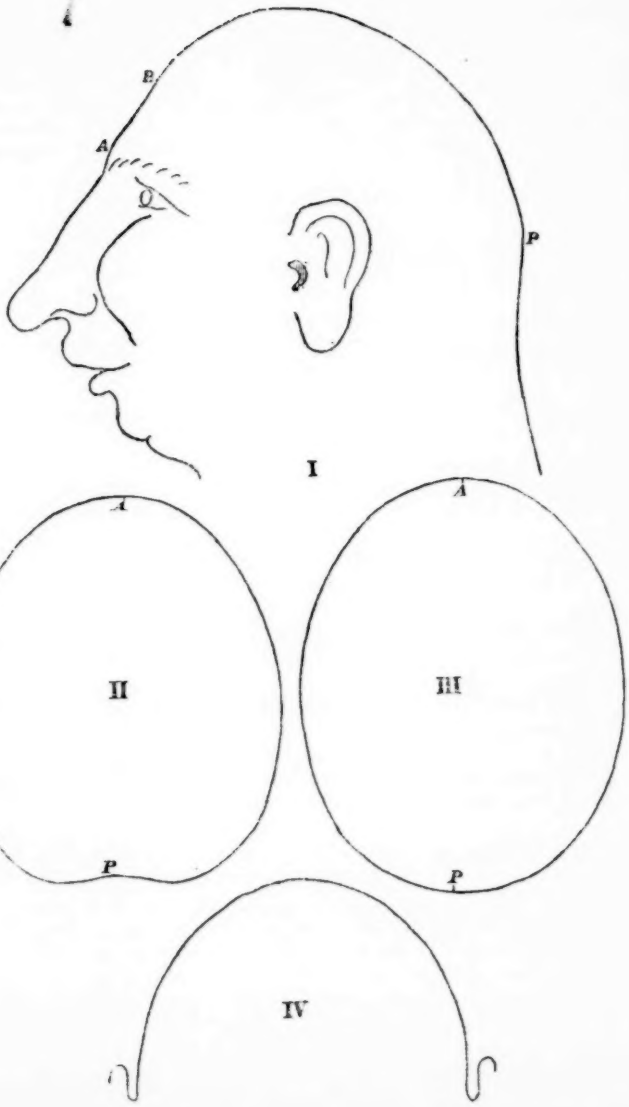
The annexed Diagrams are half life-size. They have been reduced from tracings obtained by accurately applying a strip of thin sheet-lead to the heads of the patients, in the same way as the cistometer is applied to the chest.

FIG. I.—Represents the profile view of the head of Anne G. The hairy scalp begins at B. (The features from the tip of the nose to the chin were not obtained by means of the cistometer, and have been simply added to complete the diagram. The positions of the eye and ear were obtained by accurate measurement.)

FIG. II.—Represents the outline of the head of Emily G., showing the depression of its posterior part.

FIG. III.—Represents the outline of the greater circumference of the head of Anne G. The letters A Q P correspond to the points marked by the same letters in Diagram I.

FIG. IV.—Represents the tracing obtained by applying the strip of lead to the head of Anne G., from ear to ear over the vertex.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EMBOLIC HEMIPLEGIA WITH OPTIC NEURITIS.

BY STEPHEN MACKENZIE, M.D.,

Assistant Physician to, and Lecturer on Pathology at, the London Hospital.

MATILDA D., aged 16 years, admitted into the London Hospital, under my care, March 19, 1878. She stated that she did not remember if she had had the usual infantile diseases, and that, to her knowledge, she was quite healthy until five weeks before admission. She then noticed a number of spots on her legs and feet, which lasted two weeks, and which, from her description, were probably of a purpuric nature. At the same time, she had frequent attacks of bleeding from the nose, from which she had not previously suffered. At the time of the eruption she had great pain in the shafts of her long bones, but she does not think any of her joints were red, swollen, or painful. She has had no shivering, but has suffered greatly from vomiting during her illness. She has not noticed shortness of breath. She became exceedingly weak, and had to keep her bed on this account for the two weeks preceding her admission. Her father had rheumatic fever four years ago; her mother died of consumption.

On admission, it was noted that the patient is a fair-haired girl, with blue eyes, slenderly built, sparely nourished. She lies in bed comfortably, breathing without distress; perspiring freely. Her chest is well formed, and there is natural vesicular murmur throughout. The cardiac dulness extends from third rib to two inches below nipple, and from midsternum to nipple line. The impulse is diffused, but greatest at fourth left interspace. Both sounds of the heart are intensified, but no murmur can be detected. Her pulse is 160 per minute. Hepatic dulness normal; splenic dulness not increased, spleen cannot be felt. No œdema of any part. No arthritis. The pulp of the little finger of right hand is discoloured, hard, irregular, and shrunken, looking like dry gangrene; she says the second and third fingers were like this one; they are

slightly shrivelled but not discoloured. Her temperature is 102° F., respirations 48, urine free from albumen.

March 20th.—Complains of pain in shoulder, which seems to be in muscle; it is not increased by moving the joint: no redness or swelling. At apex there is noticed a loud systolic murmur, well conducted into axilla, the second sound over pulmonary artery being accentuated.

21st.—Complains of great pain over heart, shortness of breath; no pain in joints or bones.

22nd.—I dictated, "The heart is beating with extreme rapidity. There is a thrill at the apex, which appears to terminate with the impulse. There is a distinct bruit, which from its character I judge to be combined presystolic and systolic, but the pulse is beating at 150 per minute."

29th.—It was noted, the patient is free from pain. There is no evidence of arthritis. The pulse continues to be very rapid and the temperature irregularly high. There is an indistinct thrill at the apex, where there is a loud systolic murmur conducted into the axilla.

April 9th.—Since last note, the temperature has averaged 99° in the morning and 101° at night; pulse varied from 110 to 140. She complained yesterday and to-day of pain in the calf of the left leg. On examination, this part feels denser and more bulky than that of the other leg, and its circumference exceeds that of its fellow by a quarter of an inch. No discolouration of skin. The femoral arteries can be felt pulsating, but neither popliteal nor posterior tibial arteries can be felt on either side. Heart beating at 142, its impulse is accompanied by a thrill and a murmur. The latter is mainly systolic, but it also appears somewhat of a presystolic character; the rapidity of the heart's action will not allow of a definitive judgment on this point.

April 13th.—Brawny feeling disappeared from calf, but circumference still exceeds right by quarter of an inch.

April 18th.—Complains of intense throbbing pain in left side in region of spleen. Splenic dulness not below margin of ribs, spleen cannot be felt on account of the pain. She felt sick, and shivered slightly from cold last night, at the time the pain began; her teeth did not chatter. Has vomited the last two days after every meal. No alteration in physical signs.

April 23rd.—The pain has left her side two days: still quarter an inch difference between calves of legs. No pulsation in popliteal arteries. Temperature remains high, and pulse rapid. Heart sounds as before.

April 26th.—Patient had a slight attack of epistaxis this morning. When she awoke she had severe frontal headache.

April 27th.—Complained of headache all day yesterday, and vomited several times during the day. This morning, when she got up to have her bed made, she would have fallen had she not been caught by the nurse. The nurse says that at the time she noticed the patient's face was drawn to the right side, so as to expose her teeth. There was no convulsion or abnormal movement, but it seemed as if the patient had suddenly lost control over her muscles. There was no loss of consciousness. When seen by Mr. Major Greenwood this morning, he noticed that her left arm and hand were paralysed. On telling her to grasp with her left hand she could only do so in a very feeble manner. When she raised her upper lip to display the front teeth, there was a marked deviation of the corner of her mouth to the right; she also closed the left eye less firmly than the right. The tongue protruded to the left. There was no loss of power in the left leg, apparent on examining the patient in bed.

Evening.—The paralysis of arm is much more marked this evening. She cannot raise the arm from her side, and the power of grasping is quite lost. She can only just move her fingers. There is some loss of sensation in the left arm, for, on pricking it with a pin, she does not feel nearly so well as with the right arm. There is no marked loss of power in left leg. Both popliteal arteries can be felt pulsating this evening; the accuracy of this observation was tested by counting the pulse in these situations and comparing it with that at the wrist. The patient has complained of great pain in the front and back of head throughout the day.

April 28th.—This evening patient complains of pain of a throbbing character on both sides of her head. The paralysis of the left side is much more complete, and she is literally unable to move a muscle of her left arm. Loss of power in the left leg is for the first time noticeable. The patient is able to flex the leg only with difficulty; and on attempting to extend it, she can only do so with great effort, which causes tremor of the limb. Facial paralysis very slight.

April 29th.—Patient complains of headache, and of great numbness in her left hand, where, on testing, sensation is very deficient, the patient being unable to tell which finger is touched.

April 30th.—The eyes have been examined several times before without any marked alterations being detected. The examination is always attended with great difficulty, owing to her inability to fix her eyes. To-day the right eye is atropised, and there appears to be some swelling of the disc. The left eye is difficult to examine, as the pupil is undilated, whilst the movements of both eyes render any examination inexact.

May 2nd.—Headache less. She thinks she can move her leg a little better, otherwise is about the same: temperature remains irregularly high, and pulse very rapid.

May 4th.—Had slight attacks of epistaxis yesterday and to-day. Thinks she can move the leg a little more. Complains of epigastric pain, and her tongue is coated.

May 5th.—Patient feels better this morning, and for the first time is able to move her left arm to a very slight degree. The only movement she can make is slight flexion of the forearm: she cannot move her fingers in the least. She got out of bed during the night, when the nurse's back was turned, and is reported to have fallen down. There is still a perceptible difference in the power of the left and right leg.

May 7th.—Movement of the left arm and leg about the same. She thinks she feels better in the arm.

May 10th.—No change in the paralysis. Flexion is the only movement she is able to make with the forearm. Can slightly flex and extend left leg: a tremor is excited in the left leg when the least resistance is offered to her efforts of extension.

May 20th.—Her condition has altered but little since last date. Her temperature has remained high, her pulse quick; she has had one or two attacks of epistaxis, and has complained of epigastric pain. To-day it is noted that she can use the left arm a little more. She can raise the arm slightly from the bed, but cannot use the fingers in the least.

May 22nd.—This morning, on asking her to show her teeth, it is noticed that the mouth is not drawn nearly so much to right as previously. There is scarcely any deviation of tongue. Has more movement of arm, but cannot move fingers at all. Has increased movement in leg. The cardiac dulness has increased to the right of sternum. Both optic discs now noticeably swollen, but no hæmorrhages observed.

May 23rd.—Dr. Hughlings-Jackson examined the patient's eyes, and confirmed the observation that the discs were swollen.

May 24th.—A hæmorrhage noticed in the course of a secondary retinal vessel above the disc.

May 25th.—Mr. Couper examined the eyes, and reported: "Both discs are swollen: there is much opaque exudative change in the nerve-substance of the discs, and in the adjoining retina, concealing the choroid there. No hæmorrhages are visible. The amount of prominence of the discs cannot be definitely measured on account of the unsteadiness of the eyes, but the swelling is by no means extreme, and the above appearances accord more with neuro-retinitis than with choked-disc." I made an independent observation the same day, and dictated: "The changes have decidedly advanced since my

last examination, nine days ago. Whereas on the last occasion the appearance of the fundus was filmy, and veiled as it were, now there is marked exudation, prominence of discs, distension and tortuosity of veins, with burying of vessels in exudation. There is one small streaky hæmorrhage in the left eye, opposite the disc on its outer side, running in the direction of the nerve fibres. There is an irregular and larger hæmorrhage in the upper and outer quadrant of the right eye."

May 28th.—Has more movement in arm and leg. Swelling of discs about the same, but there are more hæmorrhages. In the right eye there is one with a white centre, and there are other smaller white patches.

May 29th.—Urine to-day contains a trace of albumen. It has been examined almost daily in expectation of such an occurrence, but none has been detected previously; there was none on the 27th. No blood in the urinary deposit.

June 1st.—Patient can read without difficulty; 1½ Snellen.

June 5th.—Patient passed a bad night, being much distressed, crying a good deal, and sleeping but little. This morning she complains of nothing.

Evening.—When seen at 8.30 P.M. her condition was much the same as it had been for some time. She seemed quiet and composed, and she had been brighter, and had taken more interest in things about her during the day. She made no complaint. At 10.45 the house-physician was called to her, on account of an alarming attack of dyspnœa. He found her breathing with great difficulty, tossing about in a restless manner; her face had a very anxious expression, and her lips were blue. Stimulants were freely given, but she died at 11.30. Her temperature remained irregularly high, with slight morning remissions, and the pulse rapid, to the end of the case. The respiration became more rapid at the close, owing to the condition of the lung brought about by the failing heart.

Necropsy sixteen hours after death.

Brain.—Pia mater congested. No difference observed between the two hemispheres. Right middle cerebral artery pervious, but walls thick and lumen diminished. Brain tissue around artery stained of a yellowish colour, and slightly softer than other parts. Right corpus striatum and thalamus opticus a little softer than left but not discoloured. No changes in other parts of brain.

Heart.—Pericardium healthy, but contained an excess of straw-coloured serum. Mitral orifice constricted by large vegetations, so as to admit only two fingers. Valve thickened and covered with large warty fungating vegetations with constricted bases. Endocardium of left ventricle opaque; and attached to that covering septum ventriculorum, a small warty

vegetation, corresponding to one of the vegetations on the mitral valve, with which it came in contact when the ventricle was closed.

Lungs.—Pleuræ thickened in places, showing traces of old inflammation. No recent lymph. A considerable amount of fluid in each cavity. Lung substance dense and fleshy.

Liver.—Congested, with nutmeg appearance.

Kidneys.—Congested. There were several slightly puckered depressions on the surfaces of both, suspicious of old embolisms. No recent infarction. Capsules slightly adherent. No sign of interstitial or tubular nephritis.

Spleen.—Enlarged, weighing thirteen ounces. It was attached by its upper border to the under-surface of the left lobe of the liver, by which it was completely concealed. There were numerous infarctions, in different stages, scattered through the organ. One of large size, breaking down at its centre, in the upper part.[1]

The treatment pursued in the case may be briefly summarised by stating that salicylate of soda was at first administered with the effect of reducing the temperature, but without modifying the action of the heart. Later, quinine in 3-grain doses, combined with a grain of opium, was substituted for it. Absolute rest in the recumbent position was enjoined, to save the heart work, but it appears from the notes that the injunction was occasionally disobeyed.

Of the many points of interest in the foregoing case, I propose here to remark only upon the paralysis and optic neuritis.

The patient, quite a young girl, the subject of ulcerative endocarditis, was suddenly seized with paralysis of one lateral half of the body. There could be no doubt that the paralysis was caused by the plugging of a Sylvian artery with something detached from the surface of the diseased endocardium. Upon the embolus reaching the Sylvian artery the patient at once lost a certain degree of power over the muscles of the opposite half of the body, but she did not lose consciousness. Most authorities agree that loss of consciousness is unusual in embolism of a Sylvian artery, though Niemeyer is a marked exception.[2] Loss of consciousness may occur, and be as complete as in severe hæmorrhage, but this is unusual. The paralysis affected the face, tongue, arm, and leg on the

[1] This position of the spleen effectually prevented the enlargement of the organ being appreciated during life. From the symptoms, splenic embolism was diagnosed; but the swelling of the organ, usually noticed under such circumstances, could never be detected.

[2] 'Textbook of Practical Medicine,' 8th ed., vol. ii. p. 184.

side opposite to the blocked middle cerebral artery, but it did not affect them exactly simultaneously nor to the same degree. The facial paralysis was slight; it was chiefly of the lower part of the face, as is usual in hemiplegia, and it appeared early. The tongue was paralysed so as to be protruded to the paralysed side, but not so as to interfere with articulation. At the onset there was decided loss of power in the left arm and hand. The patient could only grasp feebly with the hand, but her doing so at all showed that, at this time, some power was conserved in the fingers. At the time when the paralysis was first noticed in the face and arm there was no loss of power in the leg, *apparent on examining the patient in bed*, a most important qualification, as a slight degree of weakness may exist undetected in the leg, if the patient cannot be made to stand or walk. It may be thought that the threatened fall when the patient was attacked was due to loss of power in the leg, but we only have the nurse's statement of her condition at the time, and the threatened fall may have been due to the shock of the embolism. About ten hours from the onset, the paralysis had deepened; the patient could not raise her arm nor grasp with her hand, and there remained only the coarsest movements of her fingers. It was now noticed that sensation, which at first was unaffected, was diminished in the left arm. On the following day the paralysis of the left arm was complete, the patient being literally unable to move a muscle of this limb. It was now noticed, too, that the left leg was distinctly weakened, and sensation in the left arm had still further diminished. Six days from its first appearance the paralysis began to decline. The leg, the last to be affected, was the first to recover power, and three days later slight movement returned in the arm. Improvement proceeded gradually and steadily, the leg recovering more and quicker than the arm. Of the individual parts of the upper extremities the upper arm recovered more than the forearm; whilst at the time of her death, five or six weeks from the commencement of the hemiplegia, she had not regained the slightest power of moving the fingers. The facial and lingual paralysis gradually disappeared. It will be thus observed that the most voluntary or specialised parts were affected the earliest and the most severely in the progress of the paralysis, and that recovery took place in an inverse order. Thus at the time of the patient's death, whilst the leg and the arm had to a very great extent recovered, the fingers, the most specialised parts of the extremities, had regained no power whatever. This sequential progress of and recovery from the paralysis illustrates very characteristically the law insisted on by Dr. Hughlings-Jackson, that "from destroying lesions of the cerebral nervous centres, parts suffer the more as

they are voluntary or special, and the less as they are automatic or general. Recovery follows the reverse order; the more automatic parts recover first."[1]

The definite sequential development of the hemiplegia appears to me to point to one of two occurrences. Either the plug—whether a detached vegetation or a piece of fibrine—did not close the channel of the artery in the first instance, but on the artery becoming subsequently plugged by the accretion of fibrine, or coagulation of the contents of the artery, the anæmia of the vascular territory became more and more complete, and the loss of function proportionately greater: or, as appears to me more probable, the embolus, consisting only of fibrine detached from vegetations or roughened endocardium, plugged the artery completely in the first instance, and the progress of the paralysis was due to the collateral fluxional œdema induced thereby. Kossuchin[2] has shown that when the circulation of an artery is suddenly arrested, there occurs ischæmia and narrowing of the vessel beyond the obstruction even to complete arrest of the circulation. Next there occurs a gradual squeezing of the blood out of the capillaries widened by collateral fluxion into the capillaries bordering on the district of free blood circulation, and this results in the production of hæmorrhages by diapedesis. The hæmorrhages do not depend alone on alterations resulting from arrest of the circulation, but also on the increased lateral blood pressure in the district bordering on the collateral fluxion, where they are noticed as early as from four to six hours after closure of an artery. I think the gradual progress of the paralysis, and the implication of the sensory functions subsequent to loss of motion, are best explained by the gradual development of the embolic process.

At the *post mortem* examination the channel of the artery was found restored, thus explaining the recovery from the paralysis. The recovery was not complete, and this was in accordance with the condition of the brain tissue supplied by the artery, which was still somewhat softened and discoloured. Had time been given, probably the paralysis would have entirely disappeared. Though no plug was found in the Sylvian artery, there could be no doubt that there had been one there. The walls showed signs of some change in them, and there was softening of the parts supplied. Embolisms were found in other organs. The complete disappearance of the plug inclines me to the belief that it was a piece of fibrine and not a vege-

[1] Clinical Lecture on a case of Hemiplegia. *Brit. Med. Journ.* 1874, vol. ii. p. 99.

[2] Zur Lehre von dem embolischen Infarcte. Virchow's *Arch.* lxvii. s. 449.

tation, for the former would more readily undergo disintegration and absorption.

With reference to the optic neuritis, I may mention that it is an extremely rare effect of embolism of a cerebral artery. My colleagues, Dr. Hughlings-Jackson and Mr. John Couper, who saw the case, and whose experiences are very large, had never previously seen neuritis from embolism; neither had I. Dr. Broadbent[1] has recorded the only case I know of where double optic neuritis occurred from embolism of a cerebral artery; and it is important to remark that in his case also the patient was the subject of ulcerative endocarditis. He writes: "I may again note the association of double optic neuritis with cerebral embolism, hazarding the suggestion that its non-occurrence in most cases may have the same explanation as the rarity of loss of sensation, and may be due to the fact that the usual cerebral embolism is of the Sylvian artery. Optic neuritis, however, goes rather with kinds of disease than with seats." Dr. Clifford Allbutt writes: "Embolism does not give rise to any definite eye mischief, so far as I know, unless it be followed by extensive cerebral softening. The contrary is stated by some writers; and I myself thought it likely that immediately after the accident, when the internal carotid was propelling blood over an area suddenly diminished, there might be some evidence of increased arterial tension, either in dilated vessels or in some degree of effusion. In one or two cases I have noticed some degree of haziness about the borders of the disc and vessels, but nothing that I could with a good conscience call a distinctly morbid state."[2] Leber[3] speaks of neuritis (papillitis) as of exceedingly rare occurrence from cerebral softening of embolic origin. He gives no case of the kind, and doubts whether, when it occurs, the neuritis is really due to the embolic process, or to the disease which gives rise to it. Galezowski[4] mentions a case where circumscribed softening of the brain, not stated to be embolic, caused optic atrophy; and mentions that Bernhardt[5] has recorded two cases of softening of brain, one due to embolism of the communicating and Sylvian arteries, with atrophy of the optic nerves. Galezowski, in another place (p. 226), writes, "When the Sylvian artery is obliterated, the central organs of vision are not necessarily affected: that is why we do not generally

[1] *Trans. of Clinical Soc.*, vol. ix. p. 68.
[2] 'On the Use of the Ophthalmoscope.' Macmillan and Co.
[3] Von Graefe und Saemisch, 'Handbuch der gesammten Augenheilkunde.' Bund v. Hälfte 2. Theil v. Hälfte 2.
[4] 'Traité Iconographique d'Ophthalmoscopie.' 1876, p. 224.
[5] 'Berliner klinisch-Wochenschrift,' 6th Sept., 1875.

find any alteration of the papilla of the optic nerve, and the vision is not disturbed. But if embolism or thrombosis occurs in the middle meningeal artery, it may induce an atrophic process in the optic nerve, corresponding to the paralysed side." He probably means of the side corresponding to the *lesion*, for he mentions a case he saw with Professor Charcot, at La Salpétrière, of a young girl between eighteen and twenty years old, the subject of incomplete right hemiplegia with aphasia, who had atrophy of the left papilla. He gives no case of neuritis, unilateral or bilateral. I have have not searched all authorities on the subject, but it is clear from those I have quoted that double optic neuritis is a very rare consequence of embolism of a cerebral vessel. It is equally important to bear in mind the rarity, and the occasional occurrence, of neuritis from this cause. There was nothing that I can perceive peculiar to this case which would account for the neuritis supervening upon embolism. It will be observed that the sight was but little disturbed; the patient complained of no defect of vision.

I am indebted to Mr. Major Greenwood for many of the notes of the case.

A CASE OF RIGHT HEMIPLEGIA, HEMIANÆSTHESIA, AND APHASIA, HAVING FOR ITS PROMINENT ANATOMICAL LESION SOFTENING OF THE LEFT LATERAL LOBE OF THE CEREBELLUM.

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THE particulars of the case which I record in the following pages are of interest, as illustrating the very anomalous occurrence of right hemiplegia, hemianæsthesia, and aphasia, accompanying a destroying lesion of a portion of the cerebellum, without the simultaneous existence of any disease in the cerebral cortex or hemispheres sufficient to account for these symptoms; and the case thus becomes of importance as bearing on the recent views put forward by certain physiologists regarding the diffusion of the functions of the brain in contradistinction to the doctrine of localisation; though, as Professor Ferrier has remarked, exceptional instances of this kind do not carry with them the same weight as would be carried by the converse of the condition above mentioned, that is, by a destruction of certain regions, in which, according to the latter doctrine, certain functions are supposed to reside, without the abrogation of such functions.

Murtagh Twomey, aged about 38 years, admitted into the District Lunatic Asylum, Cork, May 8, 1877.—The patient was unmarried, and had been for many years employed as a handy man, residing in the suburbs of Cork, his father before him having been in the same employment. Since his boyhood he was noticed to have been of an excitable disposition, and at times odd and eccentric in his habits and manners. In his youth he had a fever and subsequently became epileptic, the fits, of a mild character, seizing him occasionally. He was somewhat intemperate, and sometime since had a severer epileptic

seizure than usual, followed by right hemiplegia and aphasia, which lasted three weeks, complete recovery both of speech and motor power ensuing. His parentage disclosed no hereditary tendency to mental derangement or nervous disease. Sometime before his admission to the asylum he became unusually excited, and occasionally had paroxysms of violence; his memory was noticed to be defective, he mistook evening for morning, and delusions became developed; he fancied himself possessed of a considerable sum of money, and desired to go to London to set up a hotel; finally, he became so irascible and quarrelsome, without any provocation, and his actions betrayed such evident mental derangement, that he was sent to the asylum. On admission he was found to be talkative and restless, generally good humoured, but passionate if crossed or at all excited. His features were marked with a weak and facile expression, and he spoke in a simpering tone, an almost constant silly, self-satisfied smile overspreading his face; his memory seemed partially affected, he could however state his past history, where he came from, and what he had been employed at. When allowed to wander from any given point his conversation became quite irrational and partially incoherent, he says he is "about to receive thousands of pounds, and that he is immediately going to London." He was very averse to remaining in the asylum, and appeared to ridicule the idea of being a fit subject for detention there. Physically he was fairly healthy looking, his muscular development was poor, and, though by no means emaciated, he was rather thin than otherwise. The motor and sensory nervous systems were at this time apparently quite intact; he walked firmly and rapidly when requested, could turn sharply, and showed no indications whatever of any loss of power or incoordination in either limb; his manual grasp was normally powerful. Nothing amiss was detected in any of the bodily organs. On May 14th he was noted to have become very excitable and restless; without any reason or provocation he struck a fellow patient, and took to walking up and down the corridor of his ward, talking threateningly and noisily, with intervals of passionate crying. This paroxysm soon subsided, and he continued as above detailed, making himself useful in the ward, until the morning of June 8th, when he was found to have become, during the night, affected with right hemiplegia and complete aphasia, having gone to bed the previous night to all appearance in his usual condition. When examined in the morning, he was found to be quite unconscious, very drowsy, but not entirely comatose, as, on speaking loudly to him, he opens his eyes, and gazes around him for a few moments with an unmeaning vacant stare. The pupils are of

normal size, the irides mobile. He cannot articulate a word, nor does he emit any sound, and the right arm and leg are perfectly flaccid and helpless; sensation is completely gone over the whole of the paralysed side. The temperature in the right axilla was 101·2°, that in the left 102°. On the following day he was more drowsy, the paralytic condition being unchanged; he swallowed liquid food readily. On the 12th he was brighter, and showed some indications of returning consciousness, no change in the condition of the limbs. He smiled when spoken to, and seemed partially to recognise those around him. On the morning of the 13th he suddenly became affected with unilateral right-sided convulsions, which occurred and were repeated every five minutes. At 10.30 A.M. a spasm commenced in the ocular muscles and occipito-frontalis, travelling downwards, and seizing upon those of the paralysed arm and leg, terminating and lingering longest in the zygomatici and masseter of the right side. Forty grains of bromide of potassium, with 10 grains of chloral hydrate were at once administered, and rapidly checked the convulsions. On the 14th he appeared again somewhat improved, the attendant reporting that "he had spoken to him." I, however, did not hear him speak, and could not bring from him any articulate sound. The man continued thus, without any increase in the motor power, or any change in the condition of the muscular groups of the affected limbs, which responded to the faradaic current, for two days, his general condition being at one time favourable, and again at another worse, until the evening of June 16th, when, without any fresh or further symptoms developing, he succumbed. For some little time previous to death he perspired copiously.

Autopsy two days after death.—On incising the scalp and cutting through the calvarium, the latter was found to be thickened and dense, the diploe being replaced by osseous deposit. The dura mater was not adherent to the bone, its vessels were full of blood. The anterior three-fourths of the great longitudinal sinus contained a decolorised stringy clot which could be drawn out; the posterior fourth contained fluid blood. The dura mater was adherent to the arachnoid and pia mater over the region of the superior parietal lobule of both hemispheres by corpora pacchionii. The vessels of the arachnoid and pia mater were distended with dark-coloured fluid blood. About three ounces of sanguinolent fluid escaped during the removal of the brain. The vessels at the base of the organ were atheromatous. At the junction of the vertebrals, to form the basilar, was a thickened yellow patch on the left side of the vessel, the size of half a grain of maize; on the anterior part of the basilar there were

also slighter patches of disease. Neither of the internal carotids was effected. On the right middle cerebral were several spots of atheroma; the left was almost free, as were the dividing branches of both to their minute ramifications. On the right anterior cerebral was a small patch. No embolus or other plugging of the vessels could be detected even to the minute divisions of the middle cerebrals. The pia mater was thickened, milky, and opaque, where it bridged the fissura Sylvii and inferior aspect of the longitudinal cleft. The apices of the temporo-sphenoidal lobes and the orbital lobule were flabby, and to the touch diminished in consistence. On section the grey matter presented nothing coarsely abnormal; the white matter of the corona radiata, beyond being somewhat diminished in consistence throughout, and easily breaking down under the pressure of the finger, especially at the external boundary of the lateral ventricles presented nothing abnormal. Both opto-striate bodies were flabby and flattened, the posterior extremity of the left optic thalamus being more markedly so than elsewhere, as was best seen when the organs were looked at horizontally from behind, the latter then appearing to have lost to a certain extent its normal plumpness and rounded contour. The third ventricle was widened, and its peduncles presented a dirty yellowish colour; the commissures were intact. On repeated sections no focus of softening or other localised lesion was found to exist in either the extra- or intra-ventricular nuclei of the corpora striata. The surfaces of the hippocampal eminences were diminished in consistence, but beyond this general flabbiness of tissue (which may have been due to *post mortem* change) no localised lesion, with the exception of the above-mentioned atrophic condition of the posterior extremity of the left optic thalamus, could anywhere be detected in either cerebral hemisphere. The inferior frontal convolutions, with those surrounding them, of both hemispheres were carefully examined microscopically by repeated section, but with negative results, the grey substance of the cortex and the medullary matter subjacent being quite similar in appearance and consistence to that elsewhere, and presenting to the naked eye no appearance of disease. The left lateral lobe of the cerebellum was found to be the seat of marked disease, being so softened and broken down that a considerable gap was produced in its external border, where the tissue was entirely disorganised and destroyed. The spinal cord presented nothing abnormal to the naked eye. Portions of the brain and spinal cord were placed aside for future microscopical examination, but unfortunately got spoiled in preparing, during my absence from home, therefore I regret that, so far as the microsco-

pical examination could have thrown further light on the case, the report is imperfect. Taking, however, the coarse lesions, we have here a case presenting clinical symptoms pointing strongly to cerebral mischief, and that of a localised character, while the autopsy fails to reveal lesions, either as regards position or character, such as we would have expected to find, according to the modern doctrines of localisation. On the contrary, we have a lesion of a remote region of the encephalon, none of the ordinary symptoms of which were indicated during life. The experimental inquiry into the functions of the cerebellum has occupied the attention of many observers in different countries and at different times; and though certain differences exist, yet it would appear that since the well-known experiments of Flourens first directed attention to the influence of this organ on motor co-ordination, more unanimity exists as to the part which it plays in the scheme of the nervous system, than does that of the higher centres. It is not my intention here to enter upon the results of such experimental inquiries, it will suffice for my present purpose to indicate what they have shown the functions of the cerebellum not to be, in relation to the case under consideration. Ferrier states ('Functions of the Brain,' p. 111) that though disputed by some, it seems to be established beyond all question, that, while interfering with the mechanical adjustment against disturbance of the bodily equilibrium, lesions of the cerebellum do not cause paralysis of voluntary motion of the muscles which are concerned in these actions. He adds that it has been observed that tumours or apoplectic extravasations into one half of the cerebellum frequently coincide with hemiplegia of the opposite side of the body, but in these cases, as Vulpian has rightly indicated, the hemiplegia is not the result of the cerebellar lesion as such, but of compression of the subjacent tracts of the pons and medulla. As these decussate in the medulla oblongata, the effect of a tumour of the lateral lobe of the cerebellum is paralysis of the opposite side of the body. Lesions which do not exert such a compressing influence on the subjacent tracts do not cause hemiplegia on the opposite side of the body. How can the facts of the case above recorded be reconciled with this statement? Here there was no tumour, no apoplectic extravasation, or any lesion which could exert a compressing influence on the decussating tracts in the medulla oblongata, and yet complete paralysis existed on the opposite side of the body without any lesion which can at all account for it otherwise. Again, the cerebellum was at one time regarded as the *Sensorium commune*, or seat of common sensation. Nowadays, however, the results of experimental physiology appear to give this opinion an emphatic denial. "Flourens and

Vulpian have observed no indications of cutaneous anæsthesia in animals deprived of the cerebellum;" and Ferrier's experiments on monkeys are "in complete accordance with their results." These facts, taken with the data hitherto drawn from human pathology, appear to afford decisive evidence against the view that cerebellar lesions cause loss of tactile sensation, yet in the present instance we are met with an apparent contradiction to this complete cutaneous anæsthesia accompanying the motor paralysis. Indeed, the condition of affairs is so entirely opposed to modern teaching that I would feel inclined at once to reject the cerebellar lesion as playing any part in the production of the symptoms, without being able at the same time to offer any further explanation, were I unaware that such an authority as M. Brown-Séquard has spoken thus: ". . . that a disease located on one side of the brain can produce paralysis on the same side, be that disease in the medulla oblongata, the cerebellum, the pons varolii, or in any other part of the brain." Read by the light of such a statement, the hypothesis, therefore, cannot be overlooked that the lesion of the cerebellum may have produced the paralysis as M. Brown-Séquard would have it, by an irritation restraining and arresting the action of nerve cells, at a distance from the seat of the lesion (inhibition). If the cerebellar lesion, then, is to be credited with a causal influence, the case would appear to support two of the propositions recently enunciated by M. Brown-Séquard; for we have, in the first place, symptoms produced suddenly by a slowly and gradually developing lesion, as that in the lateral lobe of the cerebellum was; and, in the second place, we have a lesion in a remote region of the encephalon, giving rise to symptoms which are well known to be produced by diseased conditions affecting entirely different portions of the organ. These views of M. Brown-Séquard are, comparatively speaking, but recently promulgated; and should they have any foundation, not only the more modern doctrines of localisation, but also the older and hitherto firmly believed in theory of the crossed action of the cerebral hemispheres must be at once and for ever abandoned. With all respect, however, for their great originator, I hold that we cannot accept these views with the degree of proof at present forthcoming. Many of the experiments on which they were founded were made, as is well known, before those finer methods now employed by experimental physiologists were elaborated, and on animals too low in the scale to afford trustworthy comparison with similar conditions in man; and many of the pathological observations are old, and open to the same objection. However, they are now at the bar of science, and time and further research will show sooner or

later whether they shall stand or fall. Passing now from the cerebellar lesion in the case of M. T., let us glance for a moment at the condition of the brain itself. Although there was no coarse disease in the intra- or extra-ventricular nuclei of either opto-striate body, or in the convolutions (and microscopical examination is unfortunately wanting), yet it will be remembered that I have recorded a flattening of the normally rounded and plump posterior extremity of the left optic thalamus: in fact, a certain degree of external wasting of this organ. Is this condition at all significant? Does it bear any relation to the hemianæsthesia accompanying the motor paralysis? In view of the sudden and simultaneous occurrence of both these morbid conditions, as if depending on one common cause, it becomes difficult, if not impossible, to answer these questions satisfactorily. Experimental inquiry has shown, and many carefully observed cases recorded by Charcot, Türck, Bourneville, Hughlings-Jackson, Crichton-Browne, and others, indicate that the optic thalami are concerned with the function of sensation. Many cases have occurred where uncomplicated lesions of the thalamus of considerable extent were accompanied with anæsthesia, more or less complete, while in others this symptom was absent. Ferrier thinks that in the extent of the lesion lies the key of the explanation of the differences in the clinical symptoms of disease of this ganglion. Here, as I have already stated, there was no localised softening or disorganisation of tissue, merely a surface wasting, hence it is not easy to estimate its influence in the production of the anæsthesia; judging it by Ferrier's opinion it would be but slight, as the partially atrophic condition did not extend deeply, or disease affect the central masses of the ganglion. Keeping in mind, however, the morbid condition occurring elsewhere, this state of the optic thalamus, together with the probable degeneration which existed both in its histological elements and in those of the corpus striatum, which the man's previous history and condition, both bodily and mental, previous to his fatal seizure rendered likely, give some show of support to the hypothesis that the functional activity of these ganglia, already weakened, were arrested by the irritative lesion in the cerebellum, the route by which the motor impulses travel downwards from the "leading" speech centre being at the same time interrupted, aphasia resulting. It is to be remarked that such an hypothesis, while admitting that a lesion in any portion of the encephalon can give rise to constant symptoms, does not negative the existence of definite centres, the indefinitely situated lesion causing by its irritation an irradiative influence on, and a consequent inhibition of the functional activity of such centres. These hypotheses, though

they may satisfy the mind for a time, are after all mere speculations which do not throw any real light on the subject. We must be content at present with recording such cases; and, though we may be led to speculate thereon, we can only hope that extended research into now hidden domains may in time afford the real explanation which is at present concealed from our view.

Abstracts of British and Foreign Journals.

The Brain of the Sumatran Rhinoceros.—In the *Transactions of the Zoological Society*, vol. x., Professor Garrod describes (p. 411) and gives four figures (Plate lxx.) of the brain of the Sumatran rhinoceros (*Ceratorhinus Sumatrensis*), the original of which may be seen in the Museum of the Royal College of Surgeons. Professor Owen had briefly described (*op. cit.*, vol. iv., pp. 58 *et seq.*) and figured (Pl. xix.–xxii.) the brain of the Indian rhinoceros (*Rhinoceros unicornis*). This, though greater in size, is simpler than that of the Sumatran genus. "So complicated," says Professor Garrod, "and numerous are the convolutions, that the general type-plan of their disposition is, to a considerable extent, disguised. They very closely resemble the same in the Equidæ, as might have been surmised." As a guide to the apparently chaotic meanderings of the convolutions as represented in the plate Professor Garrod comes to our aid with a woodcut, representing the general direction of the sulci on the upper surfaces of one of the hemispheres (the left) of the brain. The fissure of Sylvius is stated to form an open angle, at the bottom of which are situated a number of small convolutions radiating from a point, which probably represent the Island of Reil.

The Brain of the "Cape Hunting Dog."—In the *Proceedings of the Zoological Society*, 1878, part ii. (March—April), Professor Garrod briefly describes (p. 374), but does not figure, the brain of the "Cape Hunting Dog" (*Lycaon pictus*). This, he states, is "perfectly dog-like," resembling that of the wolf (*Canis lupus*), as figured by Leuret and Gratiolet in their well-known work, 'Anatomie comparée du Système nerveux,' Pl. iv., Fig. *Loup*, Paris, 1839–57.

In the same paper there is a short description (p. 375), also unillustrated, of the brain of another member of the *Canidæ*, viz.,

Nyctereutes procyonides. This, too, is said to be "perfectly caniform." There is scarcely any tendency in the second gyrus to form an antero-superior angle, and the sulcus between gyri 3 and 4 is not quite parallel to the great longitudinal fissure, it diverging slightly from the middle line as it goes forward. In these respects the brain more resembles that of *Canis vulpes* (*vide* Leuret and Gratiolet, *op. cit.*, Pl. iv., Fig. 2, *Renard.*) than those of *C. familiaris* or *C. lupus.*

A Case of Absence of the Corpus Callosum in the Human Brain.—Dr. EICHLER, assistant in the Pathological Institue at Kiel, records in the current number of the *Archiv für Psychiatrie* (Band viii., Heft 2. s. 355), a case of the above form of arrest of development, and illustrates it by a plate (Taf. x.) of three figures. The number of recorded instances of absence of the *corpus callosum* in the human brain is very small. Dr. Sanders, when publishing a case seen by himself, states that ten such had been previously noticed—Ueber Balkenmangel in menschlichen Gehirn., *Archiv für Psychiatrie*, Bd. i. s. 128, and Taf. i.–iii., while Dr. Knox—Description of a Case of defective Corpus callosum, *Glasgow Med. Journ.*, April 1875, p. 227, mentions fourteen instances of the occurrence of this anomaly. To these should be added a case recorded by Professor Turner in the *Journal of Anat. and Phys.*, vol. xii. p. 241, of which an abstract appeared in the first number of BRAIN.

That which renders the case mentioned by Dr. Eichler particularly interesting is, that notwithstanding so serious a defect, there was during life no psychical derangement.

The Brain of the Selachia.—Dr. ROHOU publishes in the *Denkschriften der k. k. Akad. der Wissenschaften*, Bd. xxxviii. s. 43, Taf. i.–ix., Wien, 1878, an important paper, entitled *Das Centralorgan des Nervensystems der Selachier.* In the appended bibliography there is omitted a valuable paper, contributed to the *Journal of Anat. and Phys.*, vol. x. p. 76, Pl. vii., 1876, by Messrs. Jackson and Clarke, on the Brain and Cranial Nerves of *Echinorhinus Spinosus.*

Two important papers have recently been published upon the Nervous System of the Medusæ. OSCAR and RICHARD HERTWIG. *Das Nervensystem und die Sinnesorgane der Medusen.* Illustrated by ten plates. Large 4to. Leipzig, 1878.

G. J. Romanes.—Further Observations on the Locomotive System of Medusæ. *Phil. Trans. Roy. Soc.*, vol. clxvii, p. 659. With two plates.

The Brain of Procamelus Occidentalis.—Professor Cope, the American palæontologist, describes, in the *Proceedings of the American Philosophical Society*, vol. xvii. p. 49, casts, made after the manner of Professor Gervais of Paris, of the interior of the cranial cavity of *Procamelus occidentalis*. A plate of three figures illustrates the upper, lower, and a side view of one of the casts. The brain displays the character of the older type of *Ruminantia*, although not materially smaller than that of the llama—the South American representative of the camel—an animal which it equalled in general proportions. Professor Cope concludes that while the arrangement of the convolutions of the anterior lobes of the hemispheres is more simple than in any recent Ruminant, that of the middle and posterior lobes is essentially similar to that characteristic of this order of mammalia.

J. C. Galton.

Contributions to the Physiology of the Spinal Cord. Effects of Compression of the Aorta on the Circulation. Sigmund Mayer. (*Acad. d. Winersch. zu Wien Sitz.* March 14, 1878.)—Mayer, following the method of Kussmaul and Jenner, has investigated the effects of the anæmia (or, at least, oligæmia) of the spinal cord, consequent on compression of the arch of the aorta after it has given off the left subclavian, without injury to the pleura or other important structures.

The effects of compression of the aorta depend essentially on the duration. At the end of from 6 to 8 minutes' compression, the blood-pressure sinks slightly below the normal standard, and then rapidly regains it. This result is remarkable, when considered in reference to Stenson's experiment, in which, after compression of the aorta, complete motor and sensory paralysis continues for one to two minutes. We may therefore conclude that the chief bulk of the vaso-constrictor nerves in their course through the spinal cord are influenced by anæmia in quite a different manner from the motor and sensory tracts of the extremities. If we assume that it is expressly the grey matter of the spinal cord which is affected by the anæmia in Stenson's experiment, it is to be concluded that the paths for a large proportion of the vaso-

constrictor plies (which in the rabbit run in the splanchnics) stand in no functional relation to the grey matter.

The phenomena are different when the aorta has been compressed for a longer period—8 to 16 minutes. In this case, after the removal of the constriction, the blood-pressure sinks rapidly to the low grade which is observed after bloodless annihilation of the cerebral functions or section of the cord high up. After a few seconds, the blood-pressure again rises to the normal point, or even higher, sinks low again suddenly, and finally rises slowly, in a wave-like manner, to the normal point, at which it remains.

This rapid sinking of the blood-pressure in consequence of long-continued anæmia of the spinal cord is to be attributed to temporary paralysis of the spinal tracts between the cerebral vaso-constrictor centre and the peripheral muscular coats of the arteries.

If the compression is kept up for a longer period — from 20 minutes to 3 hours—and then removed, the blood-pressure sinks progressively till the circulation stops and death ensues. This result is due to a marked paralytic dilatation of the whole of the peripheral vessels. Even in this long-continued cessation of the aortic circulation, the cardiac action, respiration, and the cerebral functions continue. This fact has an important bearing on the metabolic relations between the brain and the blood, when it is considered that the circulation through the abdominal organs (liver, kidneys, &c.) may be completely stopped for hours without inducing cerebral disturbance.

The anterior transverse temporal Gyrus (*Gyrus temporalis transversus anterior*). By Professor Rich. Heschl. Vienna, 1878. Pp. 14.—The temporo-sphenoidal lobe of the human brain possesses a surface (looking inwards and upwards towards the insula) which as a rule has been little noticed. On this aspect several (occasionally as many as five) parallel gyri may be distinguished, running from before and laterally backwards towards the middle line. They may be termed the 'transverse temporal gyri.' Of these the most constant and best developed is the anterior transverse gyrus. It springs from the first temporal convolution (T_1 Ecker), about the middle of the lower margin of the fissure of Sylvius, and frequently the first temporal convolution passes completely into it in the form of an arch, so that the posterior half of the upper margin of the temporal lobe is formed by the second temporal convolution (T_2 Ecker). This arrangement occurs more frequently in the left

than in the right hemisphere (18 : 1), and more frequently in men than women (4 : 1). The anterior transverse temporal gyrus is distinguishable at a period at which none of the other convolutions are recognisable. Already at the beginning of the fifth month, it may be seen plainly as a gentle elevation of the lower margin of the still open Sylvian fissure.

H. Obersteiner.

Variable and Momentary Paralysis due to Lesions of the Grey Cortical Substance of the Brain. (*Gazette des Hôpitaux*, June 15th, 1878.)—The study of the localisation of disease in the brain, has of late emerged from comparative obscurity, thanks to the rapid strides made in experimental and clinical medicine. Although much, one might say, almost everything, is to be done, there are yet cases in which we can, from the symptoms, approach to a precise and determinate differentiation, if not of the exact point and limit of the lesion, at least of the zone and region mainly affected in one or other hemisphere. We may say that the physiognomy of a paralysis will be altogether different, according to the seat of its producing lesion. If the destructive lesion be amid the motor or anterior fibres of the bed of white substance, close to the peduncles, termed the *internal capsule*, the paralysis will be permanent, invariable, and incurable; while, on the contrary, if the lesion affect the grey covering of the convolutions, or, in other words, the cortical bed near the fissure of Rolando, the paralysis observed will be essentially variable, and probably transient, disappearing at the end of a few days. This variability and slight persistence of the paralysis, due to alterations of the superficial grey substance in the motor areas, MM. Carville and Duret have observed in their vivisectional experiments, and have explained by the possible substitution of the destroyed portions by portions still intact belonging to the same cortical bed. The following case bears out this view, and serves to confirm clinically the localisation experiments made upon animals.

G. G., male, æt. 32, admitted 30 Nov., 1877, under the care of M. Empis for chronic pulmonary tuberculosis, complicated by pneumothorax, and also having a white swelling of the right elbow. In spite of these several lesions, he seemed in good condition. In March, quite unexpectedly, peculiar phenomena occurred, which immediately pointed to tubercles of the brain. Several general epileptic attacks, leaving behind no particular trouble, neither accompanied by fever, vomiting, nor cephalalgia were the only symptoms of

cerebral lesion. These attacks occurred at intervals, and without any regularity. After the third attack the patient was seized by a complete paralysis of the left upper extremity. This brachial monoplegia occurred on April 18th, three weeks after the first epileptic attack, and at once suggested a cortical lesion, although the other characters of cortical paralysis were slow to develop themselves. Two days after the paralysis the patient, who during the night had been quite unable to move the limb, raised it without much difficulty, except that the movement was slow and without energy. The evening of the same day the paralysis was absolute, and this variability was observed on several following days. The intelligence was perfect, and the cephalalgic pain nil. The left lower limb, at first unaffected, in a few days presented a certain degree of paralysis, but these paralytic phenomena were of short duration. There was noticed at the same time a slight degree of superior facial paralysis. The paralysis of the arm was the most marked, and presented all the characters of a cortical paralysis, being partial, clearly limited during a certain time to the upper limb, variable and transitory, for at the end of fifteen days the patient fed himself with the left hand, although there still remained a certain degree of weakness on that side. There was never at any time any disturbance of sensation. Such were the principal characteristics of the clinical history of the patient. The lesion found at the autopsy completely confirmed the diagnosis of cortical paralysis. In fact the heap of tubercles near which the cerebral substance was remollescent, formed a large tract, extending along the superior edge of the right hemisphere nearly the whole length of the parietal lobe, and which bridged over the fissure of Rolando, touching the ascending frontal convolution on a level with what has been defined as the motor centre of the superior extremity. At this level especially, the tubercular mass occupied the whole thickness of the grey matter for a considerable space. A tubercle also, as large as a hazel-nut, was found in the cerebellum near its superior surface. This case corresponds closely with the vivisectional experiments of MM. Duret and Carville. In an animal the same region of the grey matter was destroyed and paralysis was produced, which lasting only a few days was more complete at one time than another; just as in our patient the destruction of a certain definite portion of the grey cortex produced an incomplete hemiplegia, very variable in intensity and which disappeared at the end of several days, leaving only a weakness in the parts affected. The epileptic attacks at long intervals previous to

the paralysis are equally explicable by vivisectional experiments, for it is well known that irritation of the cortex by electricity will produce not only the contraction of localised groups of muscles, but also more general epileptic attacks affecting as well the whole body as a part. In this case the epileptic attacks were too rare for one to study their type, but the predominance of the convulsions in one part of the body, the point of departure being well determined and always the same, as seen in epileptic hemiplegia, points to lesion of the cortical substance of the brain in its fronto-parietal region. Charcot has proposed to name these varieties Jacksonian epilepsies. Several similar cases to the above will be found in the excellent thesis of Dr. Landouzy, and in a little work by Dr. Grasset, of the medical faculty of Montpellier, entitled 'The Localisation of Cerebral Diseases.'

The recognition of these passing paralytic phenomena as being evidence of a still existing lesion, cannot any longer be ignored by the practitioner as merely theoretical speculations, and experimentalists must be credited with having at least pointed out the existence of a special zone of the surface of the brain, lesions in which entail certain locomotor disturbances, such as convulsions and paralysis.

CHARLES ALDRIDGE, M.D.

Three Cases of Softening of the Brain in the left Hemisphere, affecting the ascending Frontal Convolution or the Anterior Marginal. By Dr. UGO PALMERINI, Siena. *Archivio Italiano per le Malattie Nervose*, Sept. and Nov. 1877.—Dr. Palmerini starts by saying that the two positive acquisitions of late years in cerebral localisation have been the discoveries (1) that language is seated in the frontal lobes, (2) that there are motor centres in the cortex of the cerebrum. He then proceeds to his cases.

CASE I.

V. G., a tall man, robust, sanguine, with brachiocephalic head and low brow, thin white hair, large shaggy eyebrows, white round protruding eyes, thin cheeks, large red knotty nose, was readmitted into the Asylum at Siena in 1860. He returned in 1861, suffering from mania, with anxiety, depression, taciturnity, intellectual torpor, sighing, sleeplessness, and distaste for food. These symptoms continued for a long time, but gradually declined, so that in 1872 he was discharged cured. He remained quietly at home

for over two years; but in 1874, having again become melancholic, he was sent back to the asylum, being then sixty-six years of age. Libidinous symptoms, which were highly developed on the previous occasion, were now again marked. For the rest, his condition was as follows:—Depression, taciturnity, sluggishness of thought and will, affections torpid, and mind deficient. There was no appreciable defect of sensibility or of motion. The heart appeared to be hypertrophied and the aortic valve insufficient. The diagnosis made was progressive dementia, associated with slow atrophy of the brain.

On Oct. 26, 1876, V. G. became suddenly paralysed. His pupils were equal and normally contractile, the angle of the mouth slightly drawn to the left, the tongue unaltered. The right arm was incapable of the slightest voluntary movement. The corresponding leg was paralysed, but not so completely. He could not walk. There was no interference with motor power in any other part of the body, the symptoms gradually passing off without any phenomena of reaction. Dr. Palmerini modified his first opinion, that the case was one of cerebral apoplexy, in favour of the view that it was one of cerebral embolism. The right arm slightly recovered its power, while the patient became able to stand and to take a few steps round his bed about a month after the attack. On Feb. 15, 1877, however, he sank, after having fallen into a state of general atomy and marasmus.

Necroscopy. The meninges slightly injected; the dura mater somewhat thickened, and crying under the knife; fine opaque points on the arachnoid, especially along the longitudinal sulcus, and in the fronto-parietal regions. The sub-arachnoid fluid, increased in quantity and filling the spaces left by the change in the convolutions, is collected in little serous pouches.

The Cerebrum. The convolutions of the frontal lobes on both sides appear finer and shallower than usual, while the temporal lobes are well developed and the occipitals are normal. The marginal anterior, or ascending frontal convolution of the left hemisphere, is finer and thinner than the other, and is unmistakably atrophied. In two points of its middle third it appears shrivelled and depressed; when cut with the knife, there appears a softening, which involves the whole thickness of the same convolution, and extends into the central part of the cerebrum, reaching to the centrum ovale of Vieussens, where it dips somewhat into the medullary substance. This zone of softening is about a centimetre and a half in diameter. The rest of the grey substance of the brain appears

attenuated and discoloured. The lateral ventricles are larger than usual. Corpora striata and optic thalami normal. Extensive atheroma of all the arteries of cerebrum. Weight of cerebrum 1178 grammes, cerebellum 140, medulla oblongata 29. Total 1347.

Heart. Hypertrophy of left ventricle. Aortic valves somewhat atheromatous.

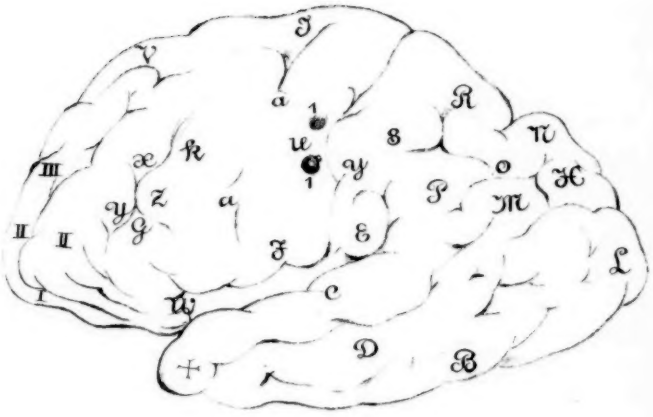
A microscopic examination of the brain shows a large quantity of fat granules and cells like drops of oil. Here and there are found numerous deposits of small size lying outside the vessels. The walls of the vessels show granules of fat and of pigment, and the vessels themselves are in some places dilated. The cells of the softened part are scanty. A few are found of normal size, and full of dark yellow granulations. The greater number are either devoid of processes or are rounded, but all are more or less charged with the yellow granules. Even in those parts of the brain which appear normal the cells are more or less charged with fat and pigment. The same is true of the cells of the spinal marrow.

Case II.

Is that of a young woman, M. M., seventeen years of age when admitted into the asylum of Siena, in 1857. She remained there twenty years till her death. The family history showed no trace of insanity. So far as appears, M. M. had been subject to epilepsy from her earliest infancy, becoming later hemiplegic and imbecile. The idiotcy probably came on gradually, while the hemiplegia is supposed to have been due to a fall in one of her epileptic attacks. In 1877, the year of her death, the following was the appearance of the girl. Low stature, small flattened head, low forehead, animal features, abundance of thick black hair, coming to the middle of the forehead, shaggy eyebrows, and a little hair on the lips and chin; black eyes, equal and normally contractile pupils; good teeth, with which she used to tear her clothes. The angle of the mouth was somewhat inclined to the left side. The right arm was paralysed, the hand powerless, strongly flexed on the forearm and pronated. The forearm, itself capable of very limited movement, flexed on the arm. The arm was capable of some movement backwards and forwards. The right leg was incompletely paralysed. She could walk on the level, dragging her right foot after her; but could not mount steps, or get into bed. Sensibility to pain was unimpaired. She was almost idiotic, having no recol-

Fig.1. *Observation.1.*

Case of V.G.



human brain one third natural size viewed from the left side. The points at which the lesion of the cortex were seated are shaded.

I. U. I. ascending frontal or marginal anterior convolution.
1.1. Points of the cortex in course of softening.

lection of home or family, and cared nothing for those about her. She took her food eagerly, and was cleanly in her person. Her vanity was considerable, and she was particularly fond of bright colours in articles of dress. Her powers of language were most rudimentary, ten vocables or so forming her stock of words. She was passionate, and would strike those who knocked against her in passing. In her fits of excitement she would tear and bite her clothes. Successive epileptic convulsions by-and-by reduced her to a state of profound coma. At last she died of a pneumonia so rapid in its nature as to rouse suspicion of pulmonary apoplexy.

Necroscopy. Cranium brachiocephalic and very small, bones very large. The frontal sinuses very large, and full of bloody serum, had pushed the orbital plates upwards and backwards, so that the frontal lobes did not reach further forwards than the cristagalli. The dura mater was thick, and showed adhesions along the longitudinal sinus. The left hemisphere was much smaller than the right. In both the convolutions were small and depressed; the sulci enlarged, and containing a somewhat turbid fluid as in pouches. The whole brain was atrophied, but the marginal convolution or frontal ascending on the left side was so much atrophied as to be only 1½ millimetre in thickness. A similar but smaller patch of atrophy is found on the corresponding part of the right side. The weight of the right hemisphere was 328 grammes, of the left 243 grammes; total 571. Cerebellum 127 grammes.

On microscopic examination, the brain cells were found smaller than usual, mostly round, some elongated; the greater part devoid of nucleus, shrivelled and full of a finely granular fluid.

Case III.

A woman, aged fifty, married, of delicate constitution, and lymphatico-nervous temperament, who had suffered from hysterical disturbances, and had been exposed to poverty and excessive toil under a burning sun, had finally to be removed to Siena in the spring of 1876. She remained there till her death in June 1877. Her illness in the asylum divides itself into two periods. In the former she showed a general depression of the feelings and of moral sensibility, with a continual sense of unhappiness; hyperæsthesia over the pit of the stomach; disturbance of the general sensibility, internal and external; and disorders of circulation, indicated, *e.g.*, by blushings, which seemed to point to affection of the vaso-motor system of nerves. The hysterical paroxysms were

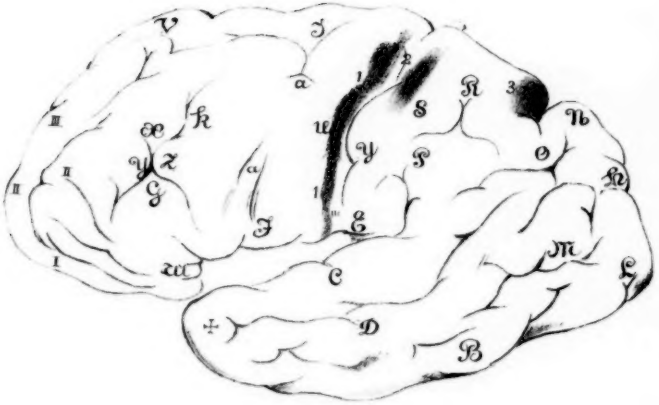
replaced by fits of depression, seemingly due to cerebral anæmia, in which she complained of vacancy and confusion of mind. This prior condition existed up to December 1876, when a graver set of phenomena made their appearance. One day she fell into a maniacal paroxysm, accompanied by tremors, violent headache, and anxiety. This passing off, she was left in a state of profound stupidity, in which questions became unintelligible to her; *she could not pronounce words*, nor stand erect, but remained sitting in whatever position she was placed. The blushings continued, along with anxiety about her health. There were marked motor and sensory disturbances. The nature of the dysphasia will be seen from the fact that, on being asked her name, she said she remembered it, but seemed unable to articulate it. When it was suggested to her, she eagerly assented, but even then could pronounce it only imperfectly. Her grief at her condition was acute. There soon appeared some paralysis of the face. The angle of the mouth was drawn to the left, and by-and-by the right arm appeared paralysed, the hand being bent on the forearm, and the forearm on the arm. Attacks of alternate excitement and vacuity succeeded each other till June, when she died. The paralysis of the right arm was more or less marked throughout.

At the necroscopy, twenty-five hours after death, the following were noted. Head dolicocephalic; meninges normal on the right side, but on the left hemisphere they were strongly and extensively adherent in the fronto-parietal region. The adhesion comprehended specially the marginal anterior or frontal ascending convolution in all its extent, and could not be removed from the brain substance without tearing the latter. This same adhesion of the membranes to one another and to the brain substance was observed for a space of about two centimetres in the sphenoid lobe. At the adherent points the brain substance was softened. The softening spread inwards as far as the centrum ovale of Vieussens, sparing only the anterior part which constitutes the frontal lobe, and the posterior which constitutes the occipital. In the left sphenoidal lobe the same softening was found. Nothing else of note appeared, except that the left optic thalamus appeared smaller than the right, but both were healthy. The weight of the cerebrum was 890 grammes, of the mesocephalon and cerebellum, 130 grammes.

Before proceding to formulate the conclusions given below, Dr. Palmerini makes some remarks on the present condition of opinion regarding the localisation of function in the brain. Dupuy, Braun and Burdon-Sanderson think the motor phenomena

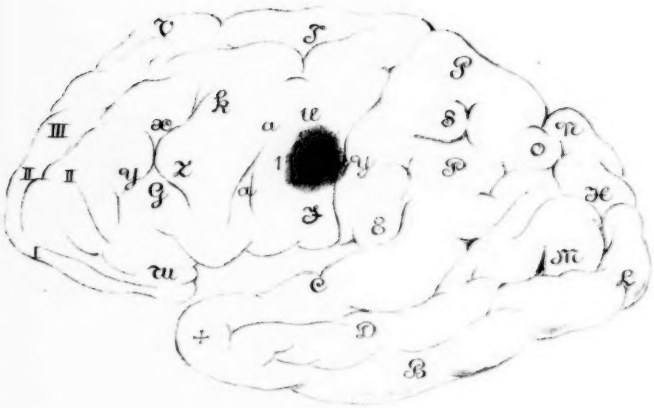
Fig. 2. *Observation 2.*

Case of M.M.



Human Brain, one third natural size seen from the left side. The diseased points are shaded.
A. A. F. ascending frontal or anterior marginal Convolution, atrophic specially at points 1.1. shaded.
2. Superior part of ascending parietal convolution R. S. E. in course of atrophy.
3. Upper and back part of the Right ascending parietal convolution in course of atrophy.

Fig.3. *Observation.3.*



Human Brain seen from left side one third natural size.
The grey substance which was the chief seat of softening as seen in the 3rd observation is shaded.
F.M.F. ascending frontal or anterior marginal convolution. 1 Seat of softening of grey substance.

due to the propagation of electricity to the nervous fibres of the corpus striatum and bulb. Schiff shows that movements obtained by exciting the brain are not such as are obtained by exciting motor centres, and considers them reflex, and says that the effects of removal of those centres are rather failure of spontaneity—are rather ataxy—than true paralysis. Thus Schiff thinks the points centres of sensibility. Tamburini attempts to conjoin these two opinions, holding the points to be centres at once of motion and of sensibility. Lussana, with whose opinions Albertoni may be associated, entirely negatives the view that the points are motor centres, like those, for instance, of the cerebral peduncles, for these reasons:—

1. In the case of true motor centres any excitation, chemical or mechanical, induces the proper movements. In the cerebral centres electric irritation only has this result.

2. Profound anæsthesia and asphyxia render electric excitation inert in the case of cerebral centres, but not so in true motor centres.

3. Certain dogs, and young animals in general, have no cerebral motor centres, but have the true motor centres.

4. Destruction of a true motor centre is followed by permanent paralysis. That of cerebral centres produces paralysis lasting only from six to eight days.

Lussana, therefore, concludes that the cortical cerebral centres are no other than organs of the will and of the divers instinctive faculties which habitually call into action the true motor centres. In this view our author concurs.

The conclusions which Dr. Palmerini draws from his three cases are the following:—

1. In the frontal ascending or marginal anterior convolution exist *psycho-motor* centres (Albertoni), which exert a manifest influence upon the voluntary movements of entire groups of muscles of the opposite side.

2. These centres correspond in man to the sites indicated by Hitzig, Ferrier, and Albertoni.

3. Lesion of these points produces persistent but incomplete paralysis, susceptible of marked exacerbation and diminution.

4. This particular form of progress of symptoms and the permanence of incomplete paralysis, particularly when preceded by exacerbation and diminution, are two clinical characters of the utmost importance.

5. The constancy of motor lesions, consisting always in phe-

nomena of paralysis and of ataxy, shows that the nature of these centres is motor (psycho-motor) and not sensory.

Case of Abscess of the Brain following Otorrhœa.—In the *Sperimentale* of 1878, Fas. 4, Dr. D. Barduzzi gives a case of otorrhœa followed by abscess of the brain. It occurred in a boy of nine years old, who had been abandoned by his parents and wandered about in great misery. All the history which could be learned was that, three years before, he had received a severe blow on the ear, which had been followed by the issue of purulent matter, and that a few days before the child had been seized with strong fever, vomiting, and delirium. On admission to the hospital he presented the symptoms of typhoid fever of a cerebral type, intense pain, dry skin, temperature from 39° to 41° C., coma, meteorism of the abdomen, muscular contractions, subsultus tendinum, contracted pupil, dulness of intellect, and slow and confused answers. There was a very scanty secretion of sero-purulent matter from the right ear; but the importance of this as a clue to the diagnosis was overlooked. The boy died forty-eight hours after admission, and it was only on a *post mortem* examination that the nature of the case was cleared up. There was found a diffused congestion of the meninges, and a limited abscess in the posterior lobe of the right side of the brain a little larger than a pigeon's egg. The white substance surrounding the abscess was somewhat softened. There was perforation and thickening of the membrana tympani, caries of the upper wall of the auditory canal, and traces of inflammation in the lateral sinus.

Where suppuration is known to exist in the internal ear, Dr. Barduzzi recommends the employment of drainage, either continuous or repeated, thrice a day, and the use of injections of salicylic acid and chloral.

Alterations in the Condition of the Lungs after Section of the Vagus Nerves. (*Lo Sperimentale*, January, 1878. O. Frey, 'Prize Memoir.')—Frey's conclusions are: 1. Division of both vagi in rabbits, dogs, cats, and guinea-pigs produces pulmonary inflammation and death, which occurs from a true broncho-pneumonia. 2. Persistent constriction of the air-passages by tying of the trachea is not followed in rabbits by any pulmonary inflammation, but only by emphysema (widening of air cells) and atelectasis. 3. Double paralysis of the recurrent laryngeal nerves causes in rabbits pulmonary changes identical with paralysis of both vagi.

In most cases, broncho-pneumonia is developed much more slowly, and is longer of inducing death. 4. Stretching of the accessory nerve and division of the vagus below the recurrent laryngeal has no influence on the lungs or liver. 5. After division of both vagi, if a cannula be inserted in the trachea, no pneumonia ensues, but only a reddening of the lung, which is to be referred to the influence of the cannula, and not to that of the vaso motor nerves which course along the vagus. Yet the animals die, as has been stated by Traube, but after a longer time than without the cannula, and the cause of death cannot be made out. 6. After injection of saliva into the lungs there ensues inflammation equal to that caused by division of both vagi. 7. Division of both superior laryngeals induced in one case pneumonia, in another not. 8. Ligature of the gullet, with division of both recurrents, produces, as Traube first showed, the same pneumonia, and in the same time, as division of both vagi. 9. Paralysis of one vagus and of one recurrent nerve induces similar pneumonia.

A. Rabagliati.

Report by Prof. Flemming on the Present Condition of the Therapeutics of Lunacy (Psychiatry), and on Asylums in Germany. (*Archivio Italiano per le Malattie Nervose.* November 1877.)—Prof. Flemming confines himself to the last ten years. After remarking that the improvements in treatment have been very considerable in that period, he refers to the great increase in the numbers of the insane. The increased supply of asylums hardly meets the demand. The increase of lunacy he ascribes to the growing pressure of civilisation, the progress of industry, the *auri insatiabilis fames*, and the increase of nervous diseases in general, with depraved hereditary influences mainly dependent on marriages of consanguinity. A special cause in England and America is said to be the mischievous effects of alcohol. In Germany, every district, every province, and every township, at least possesses one or two asylums. In the last half century eighteen or twenty new asylums have been built, nine in the kingdom of Prussia, and three at least in the Rhine Provinces, while it is contemplated to increase the size of those at Saxenberg and Mecklenburg. In many of these the insane are carefully provided, as far as possible, with the pursuits to which they had been accustomed. Accordingly, we find agricultural 'colonies' at Hildesheim in Hanover, at Kolditz and Hubertsburg in Saxony, and one in contemplation at Mecklenburg and elsewhere. Lunacy being on the increase as well in the

higher classes, private asylums have become much more numerous in the same period.

As to treatment two methods are detailed. The first consists in the complete abolition of coercive remedies. With the discussion of this method English readers are perfectly familiar through the works of Tuke, Conolly, and others. The second is described as the narcotic method (*metodo di stordimento: Betäubungsmethode*). Narcotics are used such as hyoscyamus (Greding), small doses opium (Enquelhen de Brennen), hypodermic injection of morphia (Wood), very small doses of atropine alone or in combination with opium. Allied to these are such remedies as chloroform, bromide of potassium, which diminishes cerebral congestions, and hydrate of chloral. Attention is specially directed to the hypnoticum of Prof. Preyer of Jena, which is a lactate of soda, and acts as a calmative, Preyer believing that the accumulation of lactic acid in the brain is the cause of normal sleep. This remedy is administered in doses of five to sixteen grammes by the mouth or rectum. This remedy, however, is still on its trial.[1]

Prof. Flemming has a high opinion of the value to the science of psychiatry in Germany of the frequent congresses of medical men engaged in this branch. The first of these was held in Kiel, at the suggestion of Prof. Jessen, and has become a yearly institution. The paper closes with a notice of the German periodicals devoted to this branch of science, and with a sketch of the arrangements made by the universities for its inclusion in the clinical course. It is stated that the German Empire with a population of forty millions, contains eighty-seven public asylums, with 20,700 patients, and 107 private asylums, containing 5,000 patients. These establishments are attended by 325 medical men. Twenty-six establishments are devoted to the care and instruction of idiot children.

A. Rabagliati.

Hemianæsthesia. F. Müller, Graz. (*Berl. Klin. Wochenschr.* 1878, No. 20.)—This paper contains a very carefully described case of hemianæsthesia. The patient was a man, aged 61, suffering from chronic alcoholism and aortic insufficiency. About seven weeks before admission into hospital, he complained of headache, constantly increasing in severity, and of feelings of numbness, tingling, and creeping in his right arm. In a few days these

[1] This remedy has proved of only small and doubtful utility in my hands.

symptoms were succeeded by an apoplectic seizure, with right hemiplegia and loss of speech. Speech was regained in eight days, and in fourteen days the patient was so far recovered that he could leave his room and attend to his business; he only felt a weakness in the hand and foot of the right side. He was admitted into hospital, complaining of headache and cough.

An examination of the nervous system revealed slight right hemiparesis; the movements on this side were somewhat weak, but were regular and well co-ordinated; on walking with the eyes closed there was no evidence of ataxia. There was complete right hemianæsthesia. The patient was not aware of the loss of sensation, and was quite surprised at experiencing no pain on being pricked with a pin. The hemianæsthesia was strictly limited by the median line. The bones had lost their sensibility. Pressure over the liver was slightly felt. Tickling and pinching of the upper and lower extremities were followed by reflex movements; tickling of the right external auditory meatus was without effect. The conjunctiva and cornea, and the mucous membranes of the cheek, tongue, soft palate, uvula, nostril, glans penis and anus were quite anæsthetic. There was a total loss of the muscular sense on the right side. Patient could not distinguish between objects of different weight; he was ignorant of the position of his limbs at any moment; and if told to move a limb, the movement being forcibly prevented, he would make an effort for a time and then cease, thinking the movement had been accomplished. The sense of temperature was lost on the right side. There was almost complete amblyopia of the right eye; the ophthalmoscope revealed nothing in the fundus that would account for this. The left eye was quite normal, and showed no trace of hemiopia. Hearing was so far affected on the right side that the ticking of a watch was only heard when the watch was in contact with the ear; on the left side it was heard with the watch at a distance of 50 cm. Smell was completely lost on the right side, and taste was greatly impaired over the whole right half of the tongue. The organs of sense on the left side were quite normal. Examination with the faradaic current showed complete loss of electro-cutaneous and electro-muscular sensibility; the minimal strength of current requisite to produce contraction was the same on both sides. The opening and closing shocks of a Stöhrer's continuous current battery of 30 cells caused very powerful contractions, but no pain. On applying the negative pole to the upper eyelid (the lids being closed), and the positive pole to the cervical vertebræ, the optical appearances

known as Purkinje's figures were produced on opening and closing the current—in the right eye with 6 cells, Stöhrer, in the left eye with as few as 2 cells. On stimulating the right border of the tongue, there were muscular contractions, but no metallic taste.

There is little to note in the further course of the case; the hemiparesis almost disappeared, the hemianæsthesia remained as marked as ever. The patient died three weeks after admission, of infarction of the right lung. On *post mortem* examination, a very sharply circumscribed focus of softening, of the size of a pea, was found involving the apex of the third division (posterior point of external segment) of the lenticular nucleus and the adjacent white matter. Very careful examination failed to discover any other lesion.

In discussing the case, the author quotes Pierret's statement that the hinder part of the internal capsule or the adjoining part of the corona radiata was found affected in thirteen out of the fifteen cases of hemianæsthesia in which an autopsy had been made. He further points out that the view here taken of the pathology of hemianæsthesia is in harmony with results arrived at by physiologists and anatomists, for Veyssière has shown by his experiments on dogs that hemianæsthesia is very specially associated with lesion of the hinder and upper part of the internal capsule; and Meynert has described in the same part of the capsule a bundle of sensory fibres proceeding from the occipital and temporal lobes directly downwards to the crus. The author attributes the hemiparesis to the implication of the lenticular nucleus. Lastly, he recognises the complete identity of the symptomatology of hysterical hemianæsthesia and of hemianæsthesia produced by foci of hæmorrhage or softening, the former being due to a functional, the latter to an affection of the posterior part of the internal capsule.

W. J. Dodds, M.B., D.Sc. (Edin.).

Pathological Lesions versus Motor Centres.—Dr. Maragliano has a paper in the *Rivista Sperimentale di Freniatria* (Anno IV. Fascicolo i.), on the localisation of motor functions in the cortex cerebri, studied from the clinical side. He considers that the views of Hitzig and Ferrier are substantially the same, as they agree in the relative position of the motor centres. He has collected ninety-seven cases of cerebral lesions, with a view to observe the correspondence of the symptoms during life with the parts of the brain injured. These cases are collected from French, English,

German, and Italian sources. He finds that they agree in a wonderful manner, especially with the observations of Ferrier, who gives greater extension than Hitzig to the portion of the cortex excitable to electricity.

Charcot and Pitres, basing their deductions on a smaller number of observations—forty-one in number—concluded that derangement of the movements of the superior extremity corresponded with a lesion of the middle third of the ascending frontal gyrus (gyrus centralis anterior). But Dr. Maragliano thinks that we cannot thus limit the motor centre of the arms which must cover the two upper thirds of the entire motor zone, for he finds paralysis and convulsions confined to the arms corresponding with lesions disseminated over this whole tract. He, however, agrees with Bourdon, who has made a collection of twenty cases, among which are paralysis or arrested development of a single limb, and amputations corresponding with atrophy of the cerebral cortex.

A continuation of the paper is promised, and in the meantime Dr. Maragliano gives the following conclusions:—

1. That alterations in the cortex of the cerebrum may cause alterations in the motor functions of the body.

2. That this influence upon the motor functions of the body comes exclusively from the frontal and parietal lobes.

3. That lesions of the frontal convolutions are more frequent than those of the parietal.

4. That in the frontal lobe the faculty of influencing the motor functions is confined to the ascending (gyrus centralis anterior) frontal, and to the posterior part of the first, second, and third frontal.

5. That of all the frontal convolutions the ascending gyrus is oftenest injured.

6. That in the parietal lobe the faculty of influencing the motor functions resides especially in the ascending parietal (gyrus centralis posterior).

7. And that lesions are found in the two ascending or median gyri in equal proportions.

8. That in the motor derangements confined to the upper extremity the lesion may be found in the two upper thirds of the motor zone.

9. That the motor derangements of both the limbs correspond to lesions in the two upper thirds of the two ascending or median gyri.

10. That when pronounced disturbances of the muscles of the eye are associated with motor derangements of one or both limbs, we constantly find a lesion in the inferior third of the ascending frontal gyrus.

11. That motor derangements of the superior palpebral muscle seem to correspond with lesions of the inferior parietal gyrus.

12. That the phenomena of excitation, cramps, convulsions, and other epileptiform attacks, correspond indistinctly to alterations in the ascending frontal and parietal convolutions, and to points immediately contiguous.

The Influence of Magnets upon Anæsthesia.—Drs. Maragliano and Seppilli (*Rivista Sperimentale di Freniatria*, Anno IV. Fascicolo i.) have made some investigations with a view to test the observations of Gellé, Landolt, and Charcot as to the influence of electrical currents and magnets upon anæsthesia. These observers have made the extraordinary statement that they were able to remove, by the application of magnets and metallic plates, the loss of hearing, of sight, and of general sensibility, and that in some cases of hysteria and chorea the loss of sensory power was shifted over to the corresponding spot on the previously sound side.

These experiments were repeated on three patients—two women affected with hysterical-epilepsy, and one man who became hemiplegic after a fall.

They have arrived at the following conclusions:—

1. In anæsthesia on one side, the application of the electric currents, or of metallic plates, or of a magnet, causes not only the return of general sensibility, but also that of special sense.

2. In anæsthesia of one side, of cerebral and organic origin, returning sensibility brought back by these various means extends not only to the zone of application, but also to the whole half of the anæsthetic body.

3. In hysteria, the return of the sensibility of the anæsthetic side coincides with the disappearance of anæsthesia on the sound side, and that in a zone perfectly symmetrical.

4. The duration of returning sensibility is more persistent in cases of organic anæsthesia than in that of functional anæsthesia.

5. Probably the different means that determine such effects all act through electric currents, which in their turn act either on the vaso-motor fibres, or more especially on the sensitive fibres.

6. The south pole of the magnet seems to have a greater action than the other pole.

7. Ovarian hyperæsthesia can disappear under the influence of the magnet.

The Pulsations of the Brain.—Dr. Carlo Giacomini gives in a pamphlet of forty pages[1] an account of a patient who had contracted syphilis from her husband. She had repeated attacks of inflammation in the bones of the skull and gummatous tumours, and several times recovered through the use of iodide of potassium. At last she took syphilitic osteitis, which destroyed a large portion of the table of the skull. She also lost the vomer and a portion of the palate. The overlying integuments of the scalp sloughed away, along with the portions of the skull lying under them. At the posterior part of the left frontal bone the dura mater was exposed to the extent of a five-franc piece, and the contraction of the cranial space after cicatrisation was calculated to be equal to the loss of 150 cc. of the brain-substance.

The author determined, along with Dr. Mosso, to make a special study of the motions of the brain, as observed through the dura mater. This was done by the help of Marey's cardiograph, the button of which was adjusted upon the centre of the part that was laid bare, the curves reproduced, as traced upon a piece of smoked paper, indicating by their zigzags the rising and lowering of the brain. The first comes during the systole of the heart, and indicates the wave of blood driven into the brain, distending all its vessels so as to produce an increase in the volume of the hemispheres. The descending curve coincides with the diastole of the heart when the vessels become emptied of blood, and the organ diminishes in volume.

The curves are not always equal, one out of every three or four of the cerebral pulsations rising higher than the other; this depends upon the respiration. At every inspiration the blood flows more freely from the vessels of the brain, and at this time the curve descends lowest. During the expiration the blood meets a greater difficulty in returning from the brain, hence an elevation in the curve of the cerebral pulsations.

By means of a rotatory cylinder applied to the chest to indicate the movements of the thorax, they were able to make out the exact correspondence of the expiration with the highest part of the cerebral curve, and that of inspiration with its descent. A great

[1] 'Sifilide Ossea: Esportazione di gran parte delle ossa del Cranio e della Faccia: Comunicazione fatta alla reale Accademia di Medicina di Torino dal Socio Dott. Carlo Giacomini,' &c.

increase of the pulsations appeared after compression of the jugular veins, and during deep sleep obtained by chloral. Under these conditions the pulsations were twice or thrice the size usual. There were also irregular undulations in the curves, which could not be explained in a satisfactory manner. On compression of the carotids the cerebral pulsations ceased, so that instead of a zigzag a straight line was left on the smoked card; but in a little time the pulsation returned, evidently owing to the blood supply finding its way into the vessels from the basilar artery through the circle of Willis. On compression of the jugulars the volume of the brain increased rapidly, and as rapidly descended on the pressure being withdrawn.

These observations, which were repeated during several months, are of indisputable value; but it is singular that the author never once remarks that the natural condition of the brain, save in infants, is different from what it was in the case studied, where there was an open wound exposing the organ to the pressure of the atmosphere.

The same number of the *Rivista Sperimentale di Freniatria* contains a valuable paper by Drs. Luciani and Tamburini, which details a part of their experiments on the functions of the brain made upon guinea-pigs, cats, dogs, rabbits, and apes, by the application of electricity to the denuded surface of the brain. As the paper is not finished, and the conclusions deduced from the experiments are still to come, it may be as well to defer giving a *résumé* of these laborious researches till the sequel is published.

W. W. Ireland, M.D.